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VOLUME 98 • NUMBER 5 • DECEMBER 1993

The American Historical Review

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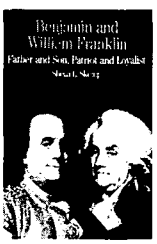
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The American Historical Review

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The American Historical Review appears in February, April, June, October, and December of each year. It is published by the American Historical Association, 400 A Street, S.E., Washington, D.C. 20003 (202-544-2422) and is printed and mailed by Byrd Press, 2901 Byrdhill Road, Richmond, Virginia 23228. The editorial offices are located at 914 Atwater, Indiana University, Bloomington, Indiana 47405 (812) 855-7609.
The *AHR* is sent to members of the American Historical Association and to institutions holding subscriptions. Membership dues: For incomes of $60,000 and above, $85.00 annually; $50,000–$59,999, $75.00; $40,000–$49,999, $65.00; $30,000–$39,999, $55.00; $20,000–$29,999, $45.00; below $20,000, students, and joint memberships $25.00; associate (nonhistorian) $35.00; life $1,200; teachers of K-12 $70.00, K-12 without *The Review* $45.00. The proportion of dues allocated to the *AHR* is $17.00. Subscription rates effective for volume 98: Class I, *American Historical Review* only, United States $52.00, foreign $65.00. Further information on membership, subscriptions, and the ordering of back issues is contained on the two pages—1(a) and 2(a)—immediately preceding the advertisements.

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In This Issue

We like to run articles that pose fresh and instructive questions. Counterfactual analysis is one method of doing so, as in the case of our first essay, which asks why Europeans enslaved Africans rather than other Europeans to supply labor for the New World. **David Eltis** questions the widely accepted assumption that African slavery was more profitable than other kinds of labor. Ideology, not economics, had to be the determining consideration, Eltis contends, for European merchants would have done better economically if they had taken advantage of existing penal institutions in Europe to obtain slaves for the Americas instead of traveling to Africa to purchase them. Yet, by the time of the transoceanic expansion, Europeans were no longer willing to enslave other Europeans. They might treat their convicts and vagrants no better than slaves but stopped short of actual enslavement. Interestingly, the northwestern European countries of England and the Netherlands, which more effectively than neighbors to the south protected their own people from forced labor, tolerated the harshest and most closed systems of African slavery in the Americas.

Eltis likewise rejects economics as an explanation for the abolition of the slave trade and looks instead to the ideas of Thomas Haskell (from his influential two-part article in the *AHR* in 1985) for a more persuasive explanation of the process by which the enslavable "outsiders" came gradually to be included in the "insider" population not subject to enslavement.

Who does not immediately recognize the painting on the cover of this issue, *The Gleaners*, or associate gleaning (the gathering of the grains that fall to the ground during harvest) with notion of village charity? For was it not the right of the village poor since time immemorial to follow the harvest and provide for their needs by gathering this residue? No, not at all, **Liana Vardi** tells us. This vision of gleaning is a recent creation, an imposition on farming communities adopted and enforced by governments beginning in the early modern period. Until the sixteenth century in France and until the advent of modern machinery in England, gleaning was a stage of the harvest proper that was performed by the farmer's family or by paid laborers. Not local communities but governments that became concerned about the disruptive potential of vagabonds and the poor insisted that communities treat gleaning as a separate activity reserved for local paupers. Rich and poor farmers alike opposed this interference with customary practice but were unable to eliminate it. Vardi traces the continuing conflict between the vision of the countryside held by officials and the actual social dynamics there.

Local conflict is also the focus of our next article, by **Margaret Lavinia Anderson**. She studies the introduction of universal manhood suffrage in Germany and its unanticipated effects. Bismarck imposed democracy in 1871, confident that local elites were unbroken and could easily manipulate local elections. Contrary to expectations, German voters were able to use the elections to register dissent and by 1912 had generated an impressive opposition. In explaining this outcome, Anderson argues that democratic elections unsettled the given patterns of deference while also binding Germans together in a national process that afforded a point of leverage from which dissidents could challenge local authorities. Voters were able to use spaces within the

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traditional political culture, the authority accorded to rules and religion, to express their dissent and ultimately to redefine both community and authority.

Continuing with the emphasis on European local history, **Marci Sortor** studies the medieval textile-producing town of Saint-Omer to test assumptions about proto-industrialization. She takes issue with the model first proposed by Henri Pirenne that sees a conflict between city and countryside and attributes a lack of competitiveness and economic decline to the institutions of the city, such as guilds and production regulations. On the contrary, Saint-Omer competed successfully with new textile manufacturers in the countryside and abroad and did so in terms of both price and product mix, all the while modifying yet continuing to use its production regulations and market privileges to protect its skilled craftspeople from downturns in the business cycle. Town manufacturers and merchants also found ways to draw on rural labor as a resource for their own industry and even marketed cloth made in the countryside. The case of Saint-Omer demonstrates that medieval cities could continue to prosper in the unsettled times of proto-industrialization and could do so by using the supposedly rigid economic institutions that they brought with them from the past.

Relations between China and the West are featured in our next two articles. The end of the Cold War has brought with it opportunities for work in the archives of Communist and post-Communist countries. In our October issue, we published the first study of the Stalinist repressions based on archival materials, a collaborative effort between Russian and Western scholars. In this issue, we present a collaborative work between an American, **Gordon H. Chang**, and a Chinese expert, **He Di**, using previously highly restricted Chinese archival materials. The essay explores the early Cold War crisis surrounding the defense of Quemoy-Matsu and other islands off the shores of the Asian mainland. Chang and He find that current interpretations by Chinese and American experts rest on assumptions about the behavior of the opposing side that do not accord with the facts. They also note that the actual course of events and the thinking of both governments at the time raise questions about the adequacy of current explanations and political "theories" about the conduct of the Cold War.

Looking back a century or two, we can see a similar inability of Westerners to understand Chinese posturing. The seeming indifference of Chinese leaders to Western technology and society in the late eighteenth and early nineteenth centuries, **Joanna Waley-Cohen** tells us, was not, as Westerners liked to believe, another proof of Chinese ingrained xenophobia and hostility to "progress." She draws on Chinese and Western sources to show that the Chinese were very interested in the West and its technology. Their studied indifference was a pose dictated by domestic political needs and a desire for independence in foreign relations. For reasons of their own, Western governments were not willing credit evidence of Chinese interest that cut against Western stereotypes, an attitude that Waley-Cohen regards as characteristic of relations between China and the West.

Our final essay, a review article by **James Hoopes**, begins with an evaluation of a collection of essays and a recent biography of Charles Sanders Peirce. Hoopes then

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goes on to discuss the importance of Peirce's philosophy for historical practice. He sees Peirce's metaphysics and semiotic as a solution for the problems addressed in the *AHR*'s continuing discussion of intellectual history and methodology. Peirce's notion of thought as an objective metaphysical category, according to Hoopes, implies that meaning may be found in history and does not need to be imposed upon it; this notion therefore provides a philosophical justification for traditional historicism. Similarly, Peirce's metaphysical monism is helpful in resolving the tension in intellectual history between the relationship of thought and action.

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(Required by 39 U.S.C. 3685)

1. **A. Title of Publication:**
 American Historical Review
 B. Publication No.: ISSN 0002-8762
2. **Date of Filing:** October 10, 1993
3. **Frequency of Issue:** Five times per year—February, April, June, October, December
 A. Number of Issues Published Annually: Five
 B. Annual Subscription Price: Class I U.S. $52.00
4. **Complete Mailing Address of Known Office of Publication:** American Historical Association, 400 A Street SE, Washington, DC 20003-3889
5. **Complete Mailing Address of the Headquarters of General Business Offices of the Publisher:** American Historical Association, 400 A Street SE, Washington, DC 20003-3889
6. **Full Names and Complete Mailing Address of Publisher, Editor, and Managing Editor:**
 Publisher: American Historical Association, 400 A Street SE, Washington, DC 20003-3889
 Editor: David L. Ransel, American Historical Review, 914 Atwater, Indiana University, Bloomington, IN 47405
 Managing Editor: N/A
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2. Mail Subscription *(Paid and/or requested)*	17,085	17,414
C. TOTAL PAID AND/OR REQUESTED CIRCULATION *(Sum of 10.B.1 and 10.B.2)*	17,085	17,414
D. FREE DISTRIBUTION BY MAIL, CARRIER, OR OTHER MEANS SAMPLES, COMPLIMENTARY, AND OTHER FREE COPIES	146	185
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2. Return from News Agents	0	0
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Europeans and the Rise and Fall of African Slavery in the Americas: An Interpretation

DAVID ELTIS

OF THE MANY POST-NEOLITHIC SLAVE SOCIETIES, those of the European-dominated Americas appear to have had the most obvious economic foundations. In the words of a recent, widely read survey, "The Slaves of the New World were economic property, and the main motive for slaveholding was economic exploitation."[1] Historians are careful to distance themselves from purely economic theories of human behavior, but on this issue the distance is usually rather short. The often bitter debates on the nature and meaning of New World slavery have produced few since Adam Smith who questioned the basic motivation of the early plantation owners. Simply put, people from one continent forced those from a second continent to produce a narrow range of consumer goods in a third—having first found the third's native population inadequate to their purpose. Even those who have wrestled with the relationship between racism and slavery have seen the racial basis of American slavery primarily as an economic phenomenon. Eric Williams, Oscar Handlin, Carl Degler, Winthrop Jordan, William McKee Evans, and others may disagree on the origins of racism but not on the origins of racial slavery in the Americas. Slaves from Africa were used to grow sugar and other plantation crops, it has been argued, because they comprised the least-cost option. Even George Frederickson, the historian who has perhaps looked furthest beyond class and economics for the sources of racism, has written that if white slavery had appeared profitable, it would have been introduced.[2] And, indeed, everything we know about early modern European commercial elites and planters would support such an interpretation. Whatever our definition of capitalism, we would expect such elites to have used the cheapest option possible within the limits of mercantilist policies.

But did this in fact happen? It has become clear in recent years that economic paradigms have limited usefulness in explaining the ending of slavery. The

I would like to thank Hilary McD. Beckles, Robin Blackburn, Timothy Joel Coates, Carl Degler, Seymour Drescher, Pieter C. Emmer, Stanley L. Engerman, Shelley Price Jones, Robert W. Malcolmson, James Pritchard, Mary Turner, and anonymous referees for extensive and helpful comments on earlier versions given at the Anglo-American Conference of Historians at the Institute of Historical Research, University of London, in July 1992, and the Department of History, Boston College, in April 1993. I have also benefited from exchanges with David and Nancy Northrup. Funding was provided by the Social Science and Humanities Research Council of Canada.

[1] Robin Blackburn, *The Overthrow of Colonial Slavery: 1776–1848* (London, 1988), 7.

[2] George M. Frederickson, *The Arrogance of Race: Historical Perspectives on Slavery, Racism, and Social Inequality* (Middletown, Conn., 1988), 194.

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number of slaveholders who voluntarily converted their slaves into wage-earning laborers in order to increase profits was not large, nor were the numbers of non-slaveholders who benefited directly from abolition. The major studies of abolition now draw rather heavily on the cultural and ideological spheres of human activity. Yet the origins of the system are in need of a similar reassessment. What follows is an attempt at a fresh examination of slavery in the Americas that suggests economics, narrowly defined, is no more capable of explaining its origins than its abolition. Put differently, the question is not why slavery per se but rather which groups are considered eligible for enslavement and why this eligibility changes over time.

Slavery until recently was universal in two senses. Most settled societies incorporated the institution into their social structures, and few peoples in the world have not constituted a major source of slaves at one time or another. The African component may dominate interpretations of slavery in the Atlantic world and, from somewhat earlier, in the Islamic world, but the more fundamental question from a longer and wider view is what separates outsiders—those who are eligible for enslavement—from insiders, who are not. Thus Nathan Huggins has answered the often-asked question of how Africans could enslave other Africans and sell them into the slave trade with the astute response that the enslavers did not see themselves or their victims as Africans. Richard Hellie has made the same point differently in writing about the efforts of slaveowners in sixteenth-century Russia to claim spurious foreign origins for themselves so that the enslaved could be held at a distance.[3] The question of why certain groups are deemed more appropriate than others for enslavement and the degree to which their status can be changed are of interest to anthropologists studying non-Western societies more often than historians and economists.[4] However, posing such questions in the European rather than the Asian, African, and indigenous American contexts promises some new insights.

If almost all societies in Europe, Africa, and the Americas, and, indeed, in the rest of the world, accepted slavery at the time of the Columbian contact, they had very different definitions of "outsider" status. In Africa and the Americas, such status might include anyone who was not part of the immediate lineage, but, more frequently, it was restricted to those not belonging to the nation or tribe—implying a somewhat wider definition of "insider." This was roughly the situation in Europe during and immediately after Roman times; by the fifteenth century, in Western Europe at least, the concept of insider had come to include all natives of the subcontinent, among whom there were some who were non-white, although very few who were non-Christians.[5] Slavery in Europe was not extensive,

[3] Nathan Irvin Huggins, *Black Odyssey: The Afro-American Ordeal in Slavery* (New York, 1977), 20; Richard Hellie, *Slavery in Russia, 1450–1725* (Chicago, 1982), 393–94. I would like to thank Stanley L. Engerman for drawing my attention to these references.

[4] See James L. Watson, "Slavery as an Institution, Open and Closed Systems," James L. Watson, ed., *Asian and African Systems of Slavery* (Berkeley, Calif., 1980), 1–15, and other essays in this volume for an introduction to the literature.

[5] A full evaluation of the amalgam of the cultural norms, behavior patterns, physical appearance, circumstances of birth, class, and economic status that determined insider-outsider status would require much more space than is available here, and precision is not, in any event, essential to the argument. Winthrop D. Jordan's *White over Black: American Attitudes toward the Negro, 1550–1812*

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but where it existed it was confined to non-Christians or natives of Africa and their immediate descendants. A similar pattern was established in the Islamic world, substituting of course "Islamic" for "Christian" in this assessment.

The line dividing insider from outsider in the European case had some flexibility even in the short term. The Spanish, though not the Portuguese or the English, had banned the enslavement of American Indians by the 1540s. Despite this move to include non-Europeans, it was religious rather than ethnic barriers against outsiders that fell first. Jews and Muslims (or at least North African and European Muslims) were accorded insider status well before Africans. But the line was never drawn strictly in terms of skin color or race, however defined. The Spanish in America may have reserved full chattel slavery for sub-Saharan Africans, but slaves in Sicily as late as 1812 were Arab or at least North African Muslims. Among insiders, the Portuguese-speaking community that ran the slave traffic to Bahia had an African component, and one of the slave ship captains of the Royal African Company in its heyday was black.[6] Moreover, the widening process was neither inevitable nor irreversible. Apart from the Russian case and well-known "second serfdom" in Eastern Europe, there was the reinstitution of serfdom in seventeenth-century Scottish coal mining.[7] These may have stopped short of or avoided slavery, but, as recently as 1802, the French reinstated full chattel slavery in their colonies a decade after it had been abolished. The key issue is thus not why slavery died out in Western Europe by the early modern period, or even why it had become confined to groups from other continents by 1500, but rather why it was not reintroduced on its old (and less exclusionary) basis when the potential of American mines and plantations became apparent.

The reasons for changes in perceptions of which groups were suitable for enslavement are not entirely clear, although a tentative explanation for the European case is offered below. The main thrust of the present argument is, first, that a study of such change should be central for anyone wishing to understand slavery in the Americas and, second, that economic motivation should be assigned a subsidiary role in the rise and fall of the exclusively African-based bondage that Europeans carried across the Atlantic. I begin with a cursory assessment of the existing literature before exploring one crucial area that this literature has tended to by-pass. Comparisons between Europe and the rest of the Atlantic world and between one European country and another form the basis of an alternative view of the issues presented in the final section.

In the last decade or so, the short description of the dividing line between insiders and outsiders has been "power relationships." Analyses along class and

(Chapel Hill, N.C., 1968) focuses on the English and Africans and puts more emphasis on the ethnic component in the insider-outsider divide than the present essay, but the early chapters still provide useful insights into European perceptions of non-Europeans in the early modern era.

[6] Great Britain, Public Record Office, Royal African Company (RAC) to Benjamin Alford, March 10, 1702, Series T70, piece 58 (henceforth, T70/58), fols. 16–17; Pierre Verger, *Flux et reflux de la traite des nègres entre le Golfe de Bénin et Bahia de Todos os Santos du XVII^e^ et XIX^e^ siècle* (Paris, 1968), 460–75. For slavery in Sicily, see Charles Verlinden, *The Beginnings of Modern Colonization: Eleven Essays with an Introduction* (Ithaca, N.Y., 1970), 27, 40.

[7] Twentieth-century Europe provides many further examples of "inward" shifts in perceptions of insider status.

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interest lines have dominated literatures on both the rise and fall of American slavery. Elites in Europe and Africa cooperated to ensure a supply of labor for the American plantations, and the shifting perceptions of a European elite—driven by protest from below in either the dominant or the slave society, according to some—brought the system to an end. Myths about slaves, other underclasses, and foreigners emanated from associations that at root and at first had a large economic component.[8]

This approach fits rather well with the more narrowly focused literature on the transition to slavery in the Atlantic world. It is frequently argued that the substitution of African slaves for the various forms of coerced native labor and European indentured servants in the plantation regions of the Americas was driven by relative costs and was thus in essence an economic decision. Debate over the mechanics of this process continues. For British North America, a combination of an elastic supply of slave labor, falling slave transportation costs, and rising servant prices governed the transition, as laid out by David Galenson, Russell Menard, and others.[9] For Brazil, Stuart Schwartz has shown, with less formal analysis, how more expensive and more productive African slaves came to replace their Brazilian Indian counterparts in the first seventy years of sugar production in the Recôncavo. Similar switches from indigenous to African labor occurred in the sixteenth-century Caribbean as well as later in South Carolina. In 1708, nearly one in four of the slave population in South Carolina was Indian, and the colony exported captives to the West Indies. Farther north, French Canadians could not afford African slaves, and their slaves remained overwhelmingly Amerindian.[10] By contrast, the southern temperate zones, closer to Africa and yielding exportable agricultural commodities of coffee and hides, had an entirely African slave labor force. Much remains unexplored, but future research will not undermine the role played by economic factors in these transitions. From a broad perspective, the large-scale traffic in slaves for plantation use focused first on northeastern Brazil, the region closest to Africa, moved next to the easternmost of the Antilles, Barbados, and had an impact on the Americas, farthest away from Africa, only

[8] William McKee Evans, "From the Land of Canaan to the Land of Guinea: The Strange Odyssey of the 'Sons of Ham,'" *AHR*, 85 (February 1980): 15–43; and for the more recent literature, Barbara Jeanne Fields, "Slavery, Race and Ideology in the United States of America," *New Left Review*, 181 (May–June 1990): 95–118. For a historiographical survey and a dissenting view, see William A. Green, "Race and Slavery: Considerations on the Williams' Thesis," Barbara L. Solow and Stanley L. Engerman, eds., *British Capitalism and Caribbean Slavery: The Legacy of Eric Williams* (Cambridge, 1987), 25–49.

[9] Russell R. Menard, "From Servants to Slaves: The Transformation of the Chesapeake Labor System," *Southern Studies*, 16 (Winter 1977): 355–90; David W. Galenson, *White Servitude in Colonial America: An Economic Analysis* (Cambridge, 1981). For a recent discussion of trends in contract lengths (an indicator of servant prices), which seem not to have increased in the 1654–1775 period, see Farley Grubb, "The Long-Run Trend in the Value of European Immigrant Servants, 1654–1831: New Measurements and Interpretations," *Research in Economic History*, 14 (1992): 167–240.

[10] Stuart B. Schwartz, *Sugar Plantations in the Formation of Brazilian Society, Bahia, 1550–1835* (Cambridge, 1985), 15–27, 51–72; Peter H. Wood, "Indian Servitude in the Southeast," Wilcomb E. Washburn, ed., *Handbook of North American Indians*, Vol. 4: *History of Indian-White Relations* (Washington, D.C., 1988), 407–09; Theda Perdue, *Slavery and the Evolution of Cherokee Society, 1540–1866* (Knoxville, Tenn., 1979), 19–35; Robin W. Winks, *The Blacks in Canada: A History* (New Haven, Conn., 1971), 1–19. Sanford Winston commented that "the relationship between the number of Indian and Negro slaves was . . . inverse," in "Indian Slavery in the Carolina Region," *Journal of Negro History*, 19 (October 1934): 436.

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later. If we leave aside the issue of alternative forms of labor, this process is consistent with trends in slave prices and transatlantic shipping costs.

But were costs as central as the extensive literature on the labor transition assumes? If coerced native, indentured or free waged European, and African slave labor were the only options open to planters, then the answer must be yes. But why should these combinations of ethnic groups and labor regimes be the only possibilities for the early plantation Americas? The extent to which other alternatives were attempted, or even considered, is ignored in the literature, despite the fact that widening the question provides insights into more than just substitution of one form of labor for another. As Eric Williams realized half a century ago, what is at stake here is not just the economics and morality of early European expansion but the foundation of relations between European and African peoples in the Americas. The current literature cannot deal easily with the questions "why no European slaves?" or "why no African indentured servants?" even though Winthrop Jordan posed the former a quarter-century ago.[11] Scholars are now less likely to counterpose slavery and freedom, or coercion and consent, especially in studying the beginning and ending of slavery in the Americas.[12] There can be no doubt that major groups subjected to slavery, such as Canaanites, Slavs, and Africans, were quickly assumed fit subjects for such status even if they were not so regarded initially. But the creation of later stereotypes does not explain why Europeans went thousands of miles to Africa for slaves in the first place. Nor does it explain why at some point between the eighteenth and nineteenth centuries, in the face of three centuries of stereotyping, the majority of Europeans and their descendants decided that Africans were no longer suitable subjects for enslavement.

If we wish to understand the origins of African slavery in the New World, or indeed in the pre-Columbian Old World, we must first explore the labor options of early modern Europeans—both those that were tried and those that were not. Second, we need to assess how close Europeans came to imposing slavery or slave-like conditions on other Europeans, and, finally, what for them set slavery apart as a status for others. These steps will help clarify the cultural and ideological parameters that at once shaped the evolution of African New World slavery and kept Europeans as non-slaves. Various forms of European forced labor were in fact tried. A comparison of these options across national boundaries reveals differences in what major European colonial powers considered feasible as labor regimes. Yet no West European power after the late Middle Ages crossed the basic divide separating European workers from full chattel slavery. And while serfdom fell and rose in different parts of early modern Europe and shared

[11] Jordan, *White over Black*, 66.

[12] Rebecca Scott, "Comparing Emancipations: A Review Essay," *Journal of Social History*, 20 (Spring 1987): 565. Most recently, see the special issue of *Slavery and Abolition*, 14 (April 1993), Michael Twaddle, ed., titled *The Wages of Slavery: From Chattel Slavery to Wage Labour in Africa, the Caribbean, and England* (London, 1993). For a similar orientation, this time at the inception of the system and centered on the study of white labor in Barbados, see Hilary McD. Beckles, *White Servitude and Black Slavery in Barbados, 1627–1715* (Knoxville, Tenn., 1989), esp. 5–10. Orlando Patterson's distinction between slave and non-slave, centered on the absolute power of the slave's master and the origin of enslavement as an alternative to death, is a useful corrective to this tendency to lump together slave and non-slave status (*Slavery and Social Death: A Comparative Study* [Cambridge, Mass., 1982], 21–27).

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characteristics with slavery, serfs were not outsiders either before or after enserfment. The phrase "long-distance serf trade" is an oxymoron. Even in the twentieth century, totalitarian states have used slave labor primarily as a punitive strategy against enemies of the state and have never instituted full chattel slavery as an economic device.

ALTHOUGH THERE IS NO EVIDENCE THAT Europeans ever considered instituting full chattel slavery of Europeans in overseas settlements, the striking paradox is that no sound economic reasons spoke against it. By the seventeenth century, the most cursory examination of relative costs suggests that European slaves should have been preferred to either European indentured labor or African slaves. And while American Indians were cheap to enslave, their life expectancy and productivity in post-Columbian plantation conditions hardly compared with that of pre-industrial or, indeed, post-industrial Europeans.

Before pursuing the issue of costs, it is worth noting that social devices used in African and Indian societies to deprive people of their liberty were fully incorporated into European societies. Judicial process sent English, French, Portuguese, and, to a lesser extent, Spanish men and women to the plantations. It also condemned Frenchmen, Spaniards, Germans, Italians, and Poles to the galleys. England, the Netherlands, and France used houses of correction to exact labor from the poor, beginning in the sixteenth century, and, in the late seventeenth century, from criminals as well. Impressment into service was also prevalent in Europe. The early literature on indentured servitude and *engagés* stressed the role of force in the acquisition of servants. In Scotland, an act of 1672 empowered coal mine owners to kidnap vagrants and set them to work.[13] If in the Americas and Africa, informal raids or more organized military actions were major ways to obtain slaves, then in Europe wars appear to have been frequent

[13] The Scottish act allowed "coal-masters, salt-masters, and others, who have manufactories in this kingdom, to seize upon any vagabonds or beggars . . . and put them to work in the coal-heughs or other manufactories, who are to have the same power of correcting them and the benefit of their work as the masters of correction houses." Cited in James Barrowman, "Slavery in the Coal-Mines of Scotland," *Transactions of the Mining Institute of Scotland*, 19 (1897–98), part 2: 119. For a fuller discussion of this and English analogies, see David Eltis, "Labour and Coercion in the English Atlantic World from the Seventeenth to the Early Twentieth Centuries," *Slavery and Abolition*, 14 (April 1993): 207–26. For the "spiriting" of servants and vagrants from England to Barbados, see Beckles, *White Servitude and Black Slavery*, 52–58. For French treatment of *engagés*, see Gabriel Debien, "Les engagés pour les Antilles (1634–1715)," *Revue d'histoire des colonies*, 38 (1951): 5–274; and for vagrants, see Andre Zysberg, "Galley and Hard Labor Convicts in France (1550–1850): From the Galleys to Hard Labor Camps; Essay on a Long Lasting Penal Institution," *Centrum voor Maatschappij Geschiedenis*, 12 (1984): 78–110, and Zysberg, "La société des galeriens au milieu de XVIII^{e} siècle," *Annales: Economies, Sociétés, Civilisations*, 30 (January–February 1975): 43–65, for which references I thank James Pritchard. For Spanish vagrants, see I. A. A. Thompson, "A Map of Crime in Sixteenth Century Spain," *Economic History Review*, 21 (August 1968): 244–50; and Ruth Pike, *Penal Servitude in Early Modern Spain* (Madison, Wis., 1983), 49–54, 67–69, 90–92. For Portuguese transportation, see Timothy Joel Coates, "Exiles and Orphans: Forced and State-Sponsored Colonizers in the Portuguese Empire, 1550–1720" (Ph.D. dissertation, University of Minnesota, 1993), which fills a large gap in the historiography of penology. There are brief references in Charles R. Boxer, *The Golden Age of Brazil, 1695–1750* (Berkeley, Calif., 1962), 140, and Stuart B. Schwartz, "The Formation of a Colonial Identity in Brazil," Nicholas P. Canny and Anthony Pagden, eds., *Colonial Identity in the Atlantic World, 1500–1800* (Princeton, N.J., 1987), 22–23.

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enough both within and between nations—setting aside the incentive to acquire and sell slaves—to ensure a healthy supply of captives. The holding of captives and the infliction of death and torture in both war and as punishment were common to European, African, and Native American societies at the time of the founding of transatlantic slave colonies. By the seventeenth century, Europeans might not enslave other Europeans, but enslavement does not appear brutal or unlikely in view of the hanging, torturing, mutilation, and burning that they inflicted on each other. European prisoners, political as well as criminal, were frequently sent to the plantations instead of to execution, and the substitution of slavery for death, explicitly recognized by John Locke, has legitimated slavery throughout history. The similarity of the mechanisms for depriving people of their liberty on all four continents is striking.[14]

From the strictly economic standpoint, one might imagine there were strong incentives for using European rather than African slave labor. The crux of the matter is shipping costs, which comprised by far the greater part of the price of any form of imported bonded labor in the Americas. First, although the trade winds of the Atlantic reduced the differential somewhat, it was normally quicker to sail directly to the Americas from Europe than to sail via Africa. In addition, mortality and morbidity among both crews and passengers (or slaves) were lower in the North Atlantic than in the South. If we take into account the time spent collecting a slave cargo on the African coast as well, then the case for sailing directly from Europe with a cargo of Europeans appears stronger again. In the 1680s, the Royal African Company (RAC) would often hire ships to carry slaves on its behalf. The hire rate was typically between £5 and £6 per slave landed alive in the Americas—a price that was understood to cover the full cost of sailing to Africa, acquiring slaves, and carrying them to the RAC's agents in the West Indies. The return cargo from the Americas was a separate speculation.[15] The cost of shipping convicts to Barbados and the Leeward Islands at this time was similar.[16] As noted below, however, ships carrying convicts, indentured servants, and fare-paying passengers always carried far fewer people per ton than did slave ships. There is little doubt that if ships carrying Europeans had been as closely packed as those carrying Africans, costs per person would have been much lower for Europeans than for Africans.

A further reason for using European rather than African slave labor derives from relative prices of African slaves and convict English labor—the nearest the English came to using Europeans as chattel slaves. (Although the discussion that

[14] For discussion of these in an African context, see Suzanne Miers and Igor Kopytoff, "African Slavery as an Institution in Marginality," in Miers and Kopytoff, eds., *Slavery in Africa: Historical and Anthropological Perspectives* (Madison, Wis., 1977). For the same phenomena in one important North American context, see William A. Starna and Ralph Watkins, "Northern Iroquoian Slavery," *Ethnohistory*, 38 (January 1991): 34–57.

[15] Kenneth G. Davies, *The Royal African Company* (London, 1957), 198. For sample rates, see PRO, T70/943, fols. 12 and 13.

[16] John C. Jeaffreson, ed., *A Young Squire of the Seventeenth Century: From the Papers (A.D. 1676–1686) of Christopher Jeaffreson*, 2 vols. (London, 1878), 1: 159; 2: 206–07. For information on costs of freighting indentured servants, compare Hilary McD. Beckles and Andrew Downes, "The Economics of Transition to the Black Labor System in Barbados, 1630–1680," *Journal of Interdisciplinary History*, 18 (Autumn 1987): 235.

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follows centers on the eighteenth century, for which the data are relatively good, scattered evidence from the seventeenth century is also consistent with the conclusions.) Unskilled male convicts from England and Ireland sold for £16 each in Maryland in the years 1767–1775 at a time when newly arrived African male slaves in the prime age group were selling for about triple this amount in Virginia and Maryland.[17] The British males worked for ten years or less, the Africans for life. If convicts and their descendants had been sold into a lifetime of service, it is reasonable to suppose that planters would have been ready to pay a higher price for them. At this higher price, the British government and merchants might have found ways to provide more convicts. Shipping costs alone would not have interfered with the process. From this standpoint, convicts could have been sold into lifelong servitude for a price little more than that for seven or ten years of labor. Thus in the absence of an improbably rapid decline in slave prices as buyers switched from Africans to Europeans, we might suppose that there were no shipping-cost barriers to European slaves forming the basis of the plantation labor forces of the Americas.

The other major cost categories in shipping Africans to the New World were enslavement, factoring (the costs of assembling a cargo prior to shipment), and distribution (the costs of selling a slave in the Americas).[18] Together, they made up about half the cost of a new slave in the Americas. According to Philip Curtin, enslavement costs in Africa were trivial. Most of the price of a slave sold to a factor on the African coast consisted of transportation costs.[19] Could this low cost at the point of enslavement explain the apparent preference for Africans? Given the opportunities in Europe for enslavement discussed above, it seems unlikely. From an economic standpoint, adapting these opportunities to produce slaves instead of merely prisoners would have been neither difficult nor costly.

Indeed, enslavement in Europe might have been less costly than its African counterpart. First, transportation costs, which loomed so large within Africa, were bound to be lower in a subcontinent where major population centers were located near navigable waters. Established routes for "chains" existed for French convicts heading to Marseilles, Toulon, and later Brest, for Spanish convicts to Cartagena, and for Portuguese to Lisbon, some incorporating travel on canals. In England, the barges carrying parish children from London to northern textile mills could have just as easily carried people to major ports. Second, population growth in Western Europe in general, and England in particular, was considerable during the era of the slave trade. Despite a net migration of 2.7 million from the mid-sixteenth to mid-nineteenth century, England's population rose sevenfold. Scholars debate the impact on the African population of the loss of 12 million

[17] Calculated from Richard Nelson Bean, *The British Trans-Atlantic Slave Trade, 1650–1775* (1971; New York, 1975), 206–08; A. Roger Ekirch, *Bound for America: The Transportation of British Convicts to the Colonies, 1718–1775* (Oxford, 1987), 71, 124–25.

[18] These categories are taken from David Turnbull's work. Turnbull had extensive experience of the nineteenth-century slave trade. See his *Travels in the West: Cuba, with Notices of Porto Rico, and the Slave Trade* (London, 1840), 403–06.

[19] Philip D. Curtin, *Economic Change in Precolonial Africa: Senegambia in the Era of the Slave Trade*, 2 vols. (Madison, Wis., 1975), 1: 156–57, 168–69, 173–77; 2: 45–53; and "The Abolition of the Slave Trade from Senegambia," in David Eltis and James Walvin, eds., *The Abolition of the Atlantic Slave Trade* (Madison, 1981), 89–91.

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people to the Americas.[20] It is certain that this number of additional emigrants from a more heavily populated Europe over the same period would have had a negligible effect. Finally, the argument that Africans could survive the epidemiological environment of the Caribbean longer than could Europeans is irrelevant here. Whatever the European-African mortality differentials, European labor always traveled to the Caribbean sugar sector as free labor or servants, never as slaves. Medical evidence would be pertinent only if European slavery had been tried and found wanting or perhaps if European labor had not been tried at all. European demographic barriers to a supply of slaves were thus no greater than those for their African counterparts.

In fact, nearly 1,000 convicts a year left Britain in the half-century after 1718. This may not seem like many compared to an African slave trade drawing 25,000 a year from Africa in the last third of the seventeenth century and rising to an average of 50,000 a year in the half-century after 1700. Yet consider that the population of England was only 7 percent of that of Europe in 1680, and if the rest of Europe had followed the English practice in proportion, 14,000 convicts would have been available. A traffic in *degredados* from Portuguese possessions to Brazil and Angola existed from the sixteenth to the nineteenth century, and the massive fortifications at Havana and San Juan, as well as Spanish outposts in North Africa, were built in part with Iberian convicts. Germanic states that lacked maritime facilities sold convicts to Italian city-states for galley service. In France, about 1,000 convicts a year arrived at Marseilles in the later seventeenth and early eighteenth centuries, but these were all male and mostly between twenty and thirty-five years of age. The potential for a large, more demographically representative traffic in French convicts is clear.

Possible sources other than convicts were numerous. Prisoners from wars, as in Africa, could have provided many additional plantation laborers. Indeed, when in rebellion, the Irish and Scots alone could have filled the labor needs of the English colonies. Nor does the above speculation fully incorporate vagrants and the poor. A properly exploited system drawing on convicts, prisoners, and vagrants from all the countries of Europe could easily have provided 50,000 forced migrants a year without serious disruption to either international peace or the existing social institutions that generated and supervised these potential European victims.[21] If such an outflow had been directed to the plantation colonies, it is also unlikely that mercantilist statesmen would have questioned either the scale or the direction of the flow.

More specifically, British convicts could have replaced African slaves in the Chesapeake, the destination of most transport ships. The first recorded shipment

[20] For different assessments, compare Patrick Manning, *Slavery and African Life: Occidental, Oriental, and African Slave Trades* (Cambridge, 1990), 38–85; and David Eltis, *Economic Growth and the Ending of the Transatlantic Slave Trade* (New York, 1987), 64–71.

[21] Gregory King estimated 600,000 adults receiving alms and 30,000 vagrants in late seventeenth-century England. "Natural and Political Observations and Conclusions upon the State and Condition of England, 1696," in Peter Laslett, comp., *The Earliest Classics: John Graunt and Gregory King* (Farnborough, 1973), 48, 57. For convicts leaving England, see Ekirch, *Bound for America*, 27. The most recent estimate of slaves leaving Africa at this period is David Richardson, "Slave Exports from West and West-Central Africa, 1700–1810: New Estimates of Volume and Distribution," *Journal of African History*, 30 (January 1989): 1–22.

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of convicts dates to 1615. Until 1770, the number of convict arrivals was at least two-thirds that of slaves in total, although in the 1730s and 1740s slave arrivals were between two and four times larger. If all whites sent against their will to the colonies, and of course their progeny, too, had been accorded the slave status of African immigrants, there is no reason why the number of white slaves in Maryland and Virginia would not have been at least as large as the actual black slave population at the end of the colonial period.[22] And to touch briefly on a related area of interest, if one purpose of the tidewater planters in bringing in Africans was to control the poor, white, non-propertied classes, enslavement of at least part of that class would seem a much more direct way of achieving the goal.[23]

A cursory examination of potential factoring and distribution costs indicates a similar pattern of benefits in Europe relative to Africa. A factoring network of sorts actually existed. French and Spanish merchants formed the link between courts and galleys. Between 1718 and 1775, the British government contracted out the collection as well as the shipping of convicts to the Americas. Companies in London came to specialize in assembling cargoes of convicts including transfers from regional jails prior to embarkation.[24] A network of other merchants in the Caribbean and mainland North America placed convicts, indentured servants, and often slaves, too, with masters. The system could surely have functioned just as well if the greater part of this labor had been enslaved Europeans rather than enslaved Africans.

IT WAS, OF COURSE, INCONCEIVABLE that any of the labor pools mentioned above—convicts, prisoners of war, or vagrants—could have been converted into chattel slaves. The barrier to European slaves in the Americas lay not only beyond shipping and enslavement costs but beyond any strictly economic sphere. The English Vagrancy Act of 1547, which prescribed slavery for "vagabonds," limited the period of servitude to two years. The act was itself repealed after only two years as unenforceable on account of its severity. Across the English Channel in

[22] Peter Wilson Coldham, *Emigrants in Chains: A Social History of Forced Emigration to the Americas, 1607–1776* (Baltimore, Md., 1992), 43–44; Abbot Emerson Smith, "The Transportation of Convicts to the American Colonies in the Seventeenth Century," *AHR*, 39 (1933–34): 233–36; J. M. Beattie, *Crime and the Courts in England, 1660–1800* (Princeton, N.J., 1986), 470–83. Perhaps 100,000 Africans arrived in the Chesapeake by 1770; Allan Kulikoff, *Tobacco and Slaves: The Development of Southern Cultures in the Chesapeake, 1680–1800* (Chapel Hill, N.C., 1986), 65–67; and Galenson, *White Servitude in Colonial America*, 216–17. No firm estimates of convict arrivals before 1718 exist, but the number was less than the 50,000 that came to the Americas in the fifty-seven years after this date. Coldham has found records of 7,500 convicts transported before 1718 from England alone (counted from *The Complete Book of Emigrants in Bondage, 1614–1775* [Baltimore, 1988]; compare Smith, "Transportation of Convicts," 238, who counted a "minimum" of 4,431, 1655–99); Scotland, Wales, and particularly Ireland supplied many more. The estimate of rough equivalency between the descendants of convicts and the black slave population takes into account the earlier arrival of convicts and the fact that rates of natural population increase were a little higher for the white population than for the black. For those who would argue that such a system would have foundered on the difficulty of control, we should note that this issue never seemed to have threatened the existing systems of long-distance penal servitude, many involving private traders and employers.

[23] Edmund S. Morgan, *American Slavery, American Freedom: The Ordeal of Colonial Virginia* (New York, 1975).

[24] Ekirch, *Bound for America*, 17–18; Coldham, *Emigrants in Chains*, 59–87.

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the sixteenth and seventeenth centuries, slavery was considered sufficiently alien that slaves brought privately to France were freed on arrival (unless headed for the galleys), prior to a 1716 law that specifically exempted them from enfranchisement.[25] In Spain, Portugal, and all Mediterranean countries by the sixteenth century, Moors and Africans could be slaves; Christians (which meant in practice Europeans, because non-Europeans who became Christian remained slaves) could not.[26] Even Jews were less likely to be enslaved in Spain as of the later Middle Ages. In 1492, they were expelled, not enslaved. Some who fled to Portugal saw their children taken from them and sent to São Tomé but not as chattel slaves. Between the late fifteenth century and 1808, the Spanish Inquisition burned at the stake 32,000 *conversos* (Jewish converts to Christianity) for false conversion. Enslavement for this group was not an issue.[27] To historians of Eastern Europe in the fifteenth and sixteenth centuries, a key question is the second serfdom, not the second enslavement. But before addressing the aversion to slavery in the face of clear economic imperatives, we need to assess how close to chattel slavery (and to the slave trade that supported it) Europeans were prepared to come within Europe and what differences existed between European nations.

As already noted, similar devices for depriving people of liberty existed in Africa, the Americas, and Europe. Yet, if on the African and American continents, these might result in enslavement—and often ownership by Europeans on both sides of the Atlantic[28]—in Europe, the same procedures fell short of creating such a status, at least for Europeans. Throughout Europe, states could take the lives of individuals, but enslavement was no longer an alternative to death; rather, it had become a fate worse than death and, as such, was reserved for non-Europeans. Europeans would accept the making of lawbreakers and prisoners into slaves only if they were not fellow Europeans. Conceptions of the insider had expanded to include the European subcontinent, whereas for Africans and American Indians, a less than continent-wide definition of insider still pertained. In a profound but scarcely novel sense, chattel slavery for Africans and Indians in the Americas was thus a function of the non-slave status that Europeans considered appropriate for themselves—a situation with historical parallels in many slave societies. Put differently, if Africans or Indians instead of Europeans had initiated the plantation system, and had possessed the means to begin a slave trade with Europe, a trade in Europeans would not have been extensive. Slavery might have been just as prevalent in the Western world but not likely confined to people from another continent. Logically, therefore, those European states least

[25] C. S. L. Davies, "Slavery and Protector Somerset: The Vagrancy Act of 1547," *Economic History Review*, 19 (December 1966): 533–49; William B. Cohen, *The French Encounter with Africans: White Response to Blacks, 1530–1880* (Bloomington, Ind., 1980), 5, 44–46.

[26] David Brion Davis, *The Problem of Slavery in Western Culture* (Ithaca, N.Y., 1966), 221–47.

[27] David Brion Davis, *Slavery and Human Progress* (New York, 1984), 95; Haim Beinart, "The Conversos and Their Fate," Elie Kedourie, ed., *Spain and the Jews: The Sephardi Experience; 1492 and After* (London, 1992), 92–122; Coates, "Exiles and Orphans," 98.

[28] For the use of West Africans and American Indian slaves in the French galleys, see Clarence J. Munford, *The Black Ordeal of Slavery and Slave Trading in the French West Indies, 1625–1715*, 3 vols. (Lewiston, N.Y., 1991), 1: 169; Paul W. Bamford, *Fighting Ships and Prisons: The Mediterranean Galleys of France in the Age of Louis XIV* (Minneapolis, Minn., 1973), 156, 165–66, 310–11.

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likely to countenance the coercion of their own citizens may well have been among the more likely to develop a system of chattel slavery overseas.

While the rejection of European slavery was, like the acceptance of its African counterpart, an unthinking decision, some states came closer to imposing chattel slavery on their citizens than did others. Convict labor, especially that sent beyond the domestic borders of the country, would seem to be the closest that Europeans came to enslaving other Europeans. The merging or increasing cooperation between state and church courts and the movement away from strictly physical chastisement of criminals in early modern Western Europe triggered experiments with transportation and penal servitude. The French, Portuguese, Spanish, and English manned many of their early exploration voyages with convicts, and the French and Portuguese opened up their galley corps to convicted felons at about the time that the Portuguese began experimenting with transporting the same group to São Tomé, Goa, and Brazil. Most West European countries, including England, contemplated or experimented with both galleys and transportation for convicts. In the end, the Spanish and Italian states sent most convicts to galleys, the English favored transportation to distant colonies, the French and Portuguese promoted both, and the Dutch, except for fleeting experiments, did neither. Eventually, the French accepted felons for use in galleys and naval bases from other European states such as Savoy, Poland, and some German principalities. In Spain, France, and Portugal, convicts worked alongside slaves from the Mediterranean littoral, Eastern Europe, Africa, and the Americas.[29]

At the lowest common denominator, two critical features kept convict labor separate from chattel slavery. First, an individual usually had to commit a crime in order to become a convict. Set laws and judicial procedure ensured that potential convicts faced less arbitrary treatment than those who fell into slavery. The power of the state over the convict and the master over convict labor was more circumscribed than that of the slaveowner over the slave. Second, criminal status was not heritable. In both French and Spanish galleys, convicts had great difficulty winning release from the galleys whatever the term of their sentence, their property was sold upon conviction, and they had no standing in law. But Europeans, except for some adherents to the Greek Orthodox faith, were not technically slaves. Unlike Turks, Muslims, Russians, and Africans who had been specially purchased to supplement the galley corps, they could not be resold, and their status was not transmitted to their progeny.

It is striking that the more strenuous work—specifically, the farthest inboard position on the oars in galleys—was reserved for slaves, not convicts. And, when the useful life of oarsmen came to an end and the state wished to reduce expenses, old slaves were either returned to their state of origin on the basis of treaties, sold at European slave marts, or, if African, sometimes taken to the French West Indies to be sold. Convicts who managed to obtain a discharge from the French

[29] Coates, "Exiles and Orphans," 32–67; Emile Campion, *Etude sur la colonisation par les transportes anglais, russes et français* (Rennes, 1901), 44–45; Beattie, *Crime and the Courts in England*, 450–519, esp. 470–74; John H. Langbein, *Torture and the Law of Proof: Europe and England in the Ancien Régime* (Chicago, 1977), 27–44; Zysberg, "Galley and Hard Labor Convicts in France," 78–110; Pike, *Penal Servitude in Early Modern Spain*, 3–26; Pieter Spierenburg, *The Prison Experience: Disciplinary Institutions and Their Inmates in Early Modern Europe* (New Brunswick, N.J., 1991), 259–62.

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galley corps also ended up in the French Americas, but they were always sold as indentured servants. Portuguese *degredados* could be paid by the state for their labor and in São Tomé and Angola had sufficiently different status from slaves that most of them were involved in the slave trade that supplied São Tomé and Brazilian plantations.[30] The most that can be said by way of comparison is that the spread of African slavery in the Americas coincided with the spread of forced labor in punishment systems within Europe (transportation in the English case). But no one was in any doubt about the distinctions between the two.

Nevertheless, interesting differences between states emerge in the treatment of convicts. Penal servitude became a feature of the Continental European systems—effectively for life in the case of the French and Spanish. The English imposed neither lifelong banishment nor much penal servitude; the Bridewell houses held few long-term prisoners and did not always have a labor regime.[31] In the Netherlands, penal servitude supplemented, then replaced, banishment, although the Dutch did hold long-term prisoners who had been incriminated, as opposed to convicted. They also used torture after that practice was discontinued in England, where in fact it had never been part of the Common Law.[32] For the English, transportation meant shipment across the Atlantic with no right of return for seven or, for those reprieved from death sentences, fourteen years. Once in the Americas, the vast majority of prisoners were sold into seven-year terms of servitude, but the sentence itself did not provide for this.[33] Prisoners with resources could in fact buy their freedom at the point of disembarkation and, as long as they stayed away from England, convert their sentence to temporary banishment. The servitude that accompanied transportation was actually a device to pay its costs and, apart from a longer term, did not differ materially from that voluntarily experienced by non-convict indentured servants.[34] In Portugal, where the state bore the cost of transportation or imposed it on others, the sentence constituted exile, with the labor required varying according to the needs of the state. The system was remarkably flexible in alternating between galleys and transportation to the colonies, with or without forced labor.[35]

Vagrants formed a larger group than convicts in every European country, and

[30] Coates, "Exiles and Orphans," 57, 98–99; Bamford, *Fighting Ships and Prisons*, 139–52, 250–71; Pike, *Penal Servitude in Early Modern Spain*, 8–14. The term "slavery" is occasionally used loosely in the penology literature. J. Thorsten Sellin in his widely read *Slavery and the Penal System* (New York, 1976) uses the term to cover any form of coerced labor. Spierenburg, *Prison Experience*, refers ambivalently to the "bondage" of convicts throughout his book (see especially page 10). Such usage skates over distinctions important to the issue of slavery in the Americas.

[31] Spierenburg, *Prison Experience*, 1–68, 259–76; Zysberg, "La société des galeriens," 43–75; Bamford, *Fighting Ships and Prisons*, 173–83; Coates, "Exiles and Orphans," 71–72. In Spain, sentences for *forzados* were limited to ten years in 1653, although, as noted above, detention beyond the formal term was common (Pike, *Penal Servitude in Early Modern Spain*, 7–8).

[32] Spierenburg, *Prison Experience*, 41–60, 143–47; P. Spierenburg, "The Sociogenesis of Confinement and Its Development in Early Modern Europe," *Centrum voor Maatschappij Geschiedenis*, 12 (1984): 38. For the ending of torture, see Langbein, *Torture and the Law of Proof*, 10–12, 50, 134–35.

[33] Ekirch, *Bound for America*, 16–21.

[34] The longer term, equivalent to a lower price, might have reflected the greater likelihood of convicts escaping, being dishonest, a more diverse mix of ages and sexes than was the case with regular indentures, or some combination of these factors. Formal analysis has yet to be carried out on convict labor.

[35] Coates, "Exiles and Orphans," chap. 5; Boxer, *Golden Age of Brazil*, 140.

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the destitute poor, dependent on state aid, a larger group again. From the mercantilist standpoint, the productive potential of these groups in a slave society was enormous. With the increasing breakdown of church-sponsored charity systems from the mid-sixteenth century on, workhouses mushroomed in northwestern Europe, especially England, Holland, and France—three of the leading early plantation powers.[36] Penal servitude for felons was grafted onto this system in the eighteenth century.[37] Hardening attitudes toward the poor were seen throughout Western Europe in the early modern period, although the exaction of labor in return for relief appeared first in Protestant areas. The Dutch system of workhouses was the most efficient and became a model for the rest of Europe. It was greatly admired, though never wholly emulated by the English elite, and it was also the most generous.[38] The English subjected their vagrants to compulsory labor on pain of whipping and imprisonment, expected their poor to work, and sent indigent children, forcibly separated from their parents in earlier times, to masters at home and in the colonies—until the twentieth century, if we include those sent from the Dr. Barnardo homes for orphaned and destitute children. Yet the inefficiency of local government was such that the numbers caught in the system were likely modest.[39] Centralized administrations in Spain, however, ensured the impressment of vagrants and gypsies into state arsenals (naval dockyards and armament factories) as late as the second half of the eighteenth century. In France, 7 percent of the galley corps at the time of its merger with the navy in 1748 were vagrants, but, as the needs of the galleys declined after 1700, this group was more likely to be sent to the West Indies.[40] Overall in Europe, those sent to the Americas or put in the galleys formed a small share of the poor receiving relief; the fate of those sent to the Americas was invariably indentured servitude, not slavery; and the poor who could not avoid the state system were probably treated less harshly in the Netherlands and England (though for different reasons taken up below) than in France and Spain.

Prisoners taken in the course of European military action may be divided into two groups for present purposes. Those captured in the act of rebellion against established government could expect death if they were leaders, or banishment if they were deemed followers, but never enslavement. The English sold many Irish in Barbados during the seventeenth century after military campaigns in Ireland but always as indentured servants with a maximum term of ten years. In France in 1662, nearly five hundred rebellious Boulonnais were sent to the galleys, not into slavery, despite the inability of French merchants at the time to sustain an

[36] Spierenburg, "Sociogenesis of Confinement," 9–77, esp. 31–32.

[37] Spierenburg, *Prison Experience*, 12–86; Beattie, *Crime and the Courts in England*, 492–500.

[38] Spierenburg, "Sociogenesis of Confinement," 24; Simon Schama, *The Embarrassment of Riches: An Interpretation of Dutch Culture in the Golden Age* (New York, 1987), 174–75, 570–79.

[39] Dorothy Marshall, "The Old Poor Law, 1662–1795," *Economic History Review*, 1 (April 1937): 39; George Meriton, *A Guide for Constables, Churchwardens, Overseers of the Poor . . .* (London, 1669); *Statutes at Large*, 43 Eliz. c. 2. Smith, "Transportation of Convicts," 244, indicates vagrants among a group going to Virginia in the 1660s.

[40] Pike, *Penal Servitude in Early Modern Spain*, 67–69; Zysburg, "La société des galeriens"; Bamford, *Fighting Ships*, 260; Christian Huetz de Lemps, "Indentured Servants Bound for the French Antilles and Canada in the Seventeenth and Eighteenth Centuries," in Ida Altman and James Horn, eds., *"To Make America": European Emigration in the Early Modern Period* (Berkeley, Calif., 1991), 188–89.

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African slave trade to the French Americas. A second group consists of those captured in conflicts between states. At least until the French Revolutionary wars, prisoners of war were the best treated of all the European groups considered here. Detention followed by prisoner exchanges or ransoming was common. English, Dutch, and French prisoners could be consigned to the galleys in Spain, but slave status there was reserved for non-Christians. Even the latter group, however—in the case of North Africans enslaved in French and Spanish galleys—had some prospect of release in exchange for Christians held by rulers of Algiers, Tunis, and other Mediterranean Muslim powers.[41] Prisoner exchanges seem to have been less common in Africa and the Americas, where captives were more likely to be killed, absorbed into the captor's society, or traded away. Some might see the trading of captives as the distorting impact of a European-generated market for slaves, but the key point is that this option was also open to commanders of European armies and they did not use it.

Finally, we might note the large differences in transatlantic shipping conditions for Europeans and for Africans. Convicts and soldiers who had been held in confined circumstances prior to embarkation could suffer as high a shipboard mortality on the Europe-America route as slaves carried from Africa to the Americas.[42] There is, however, no evidence that any European transatlantic voyagers, whether steerage passengers, convicts, soldiers, or indentured servants, were ever subjected to conditions that were the norm on slave ships. Thus English and Irish alike traveled in convict ships that rarely carried in excess of 150 prisoners and on average fewer than 100. Far fewer persons-per-ton were carried on ships dedicated to convict transportation than on slave ships. Crowding was three or four times as severe on the slave ships from the seventeenth to the early nineteenth century, and only galley ships, rarely out of sight of land and never at sea in bad weather, matched the human density of a slave ship. While this difference may not have had much impact on mortality, which was not particularly affected by crowding, it may well say something about the views European merchants, and ultimately European societies, held regarding the status of different migrant groups. States, merchants, and consumers of plantation produce all stood to gain from shipping convicts in slave-like conditions.[43]

[41] Alfred Vagts, *A History of Militarism: Civilian and Military* (New York, 1959), 113–14; Bamford, *Fighting Ships and Prisons*, 176–77, 261–64. Bruno S. Frey and Heinz Buhofer (in an article drawn to my attention by Stanley L. Engerman) argue that the relatively good treatment of prisoners of war came from their market value and, by implication, that the European–North African differential arose from a relative lack of interest on the part of the North African powers in buying back low status prisoners ("A Market for Men, or There Is No Such Thing as a Free Lynch," *Journal of Institutional and Theoretical Economics*, 142 [1986]: 739–44). This approach can explain much, but the interesting question is why, if, for instance, neither the English nor the Algerians would buy back all their prisoners from the Spanish in the late sixteenth century, and both groups were sent to the galleys because of their lack of value on the exchange market, only the Algerians were enslaved. Likewise, to take one of their own examples, the need for labor can explain fluctuations in the treatment of death camp prisoners over the course of World War II in Germany but not the existence of the death camps themselves.

[42] See Raymond L. Cohn, "Maritime Mortality in the Eighteenth and Nineteenth Centuries: A Survey," *International Journal of Maritime History*, 1 (June 1989): 159–91, and the literature cited there.

[43] The earliest Naval Office lists are in PRO, CO 33/13 and CO 142/13. Beckles, *White Servitude and Black Slavery*, 62–67; Ekirch, *Bound to America*, 98–100; John Mcdonald and Ralph Shlomowitz, "Mortality on Convict Voyages to Australia, 1788–1868," *Social Science History*, 13 (Fall 1989):

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An almost tangible barrier thus prevented Europeans from becoming chattel slaves unless they were captured by non-Europeans. In many African societies, shared ethnicity and language might even mean an increased likelihood of enslavement—at least for women and children—given the focus on kin groups and their expansion through absorption of outsiders. In the Americas, slave raids of one Iroquois nation on another were not uncommon. But in Europe, even the most degraded member of society was spared enslavement. Only in the rather limited case of Amerindians in the Spanish Americas was such treatment extended to non-Europeans. This barrier was somewhat akin to the Muslim bar against the enslavement of non-Muslims—not in the sense that the basis of enslavement was religious but rather that in both Muslim and Christian societies slavery came to be mainly African despite the fact that in both, slaves often converted to the faith of their owners.[44]

But if all Europeans shared this attitude toward the insider, it is also clear from the above that some societies came closer than others to countenancing conditions that approached slavery for their own citizens. English indentured servants and convicts on average served less than seven years. Effective life sentences, though sometimes for exile without labor, were common for Spanish and French convicts, and at least some French *engagés* were sent to the Americas by the state with a status little different from convict labor. Likewise, slave labor involving non-Europeans was much more common in southern Europe than in the north. The slave markets in Europe from the Middle Ages until to the eighteenth century were on the shores of the Mediterranean, not northwestern Europe. Given the large African slave population in Lisbon, it is hard to visualize a Portuguese counterpart to an incident in Middelburg in 1596 in which 130 African slaves were restored "their natural liberty" on the grounds that slavery did not exist in Zeeland.[45] Nor is it easy to imagine equivalent action in any part of the non-European world—for example, North Africa—if the slaves had been European. Yet there are abundant instances of ethnocentric attitudes in sixteenth-century Holland, France, and England. Moreover, the greater the inability of the European to think in terms of slavery for other Europeans, the more he or she was likely to contemplate coerced labor for non-Europeans. In a further paradox, it is also possible that the more often coercion is seen to be unconscionable for

285–313. For slave ships, see Eltis, *Economic Growth and the Ending of the Transatlantic Slave Trade*, 135–38, and "Productivity in the Slave Trade: A Comparative Assessment" (unpublished ms., 1993). The ships that carried the Monmouth prisoners to Barbados in 1685–1686—that is, those men who were not hanged, drawn, and quartered—averaged a density of 0.72 prisoners per ton and reported less than 10 percent mortality during the voyage. Slave ships at this time embarked 2.5 persons per ton and averaged a 20 percent loss. The higher mortality on slave ships is likely explained by the greater length of the slave voyage rather than greater crowding among those on board.

[44] Bernard Lewis, *Race and Slavery in the Middle East: An Historical Enquiry* (New York, 1990), 5–15, 54–61; Davis, *Slavery and Human Progress*, 32–51.

[45] A. C. de C. M. Saunders, *A Social History of Black Slaves and Freedmen in Portugal, 1441–1555* (Cambridge, 1982), 59; W. S. Unger, "Bijdragen tot de geschiedenis van de Nederlandse slavenhandel," *Economisch-Historisch Jaarboek*, 26 (1956): 136. For this last reference and translation, I thank Pieter C. Emmer. For a similar incident in Bordeaux in 1571 paralleling that in Middelburg, see Charles de La Roncière, *Nègres et Négriers* (Paris, 1933), 15–16.

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people like oneself and appropriate for others, the more likely that coercion for anyone will eventually be questioned.

THERE ARE TWO ISSUES TO BE ADDRESSED NEXT. One is the genesis of what from a transatlantic, if not global, perspective is the rather exceptional European attitude toward slavery in the early modern period—specifically, its association with the concept of race. The second is the intra-European differences in attitudes toward coerced labor that evolved and persisted into the era of abolition. Indeed, those countries that had developed the strongest antipathy to curtailment of liberty for their own people not only had the least inhibitions about creating an overseas labor system using non-European slaves but were eventually to be in the vanguard of the movement to abolish slavery world-wide. State-enforced abolition of slavery, as well as the conception of a world without slavery, originated first in Europe.[46] These issues raise large questions, and the answers must be somewhat speculative. These speculations incline not so much toward chauvinism or ethnocentrism—at least, not at root—but rather toward longer-term economic and ecological factors that separated Western Europe from the rest of the world before the modern era.

As already noted, the difference between Europe and most of the rest of the world on the slavery issue in the early modern period was the conviction that only non-Europeans could be enslaved. Technology gave Europeans the power to sail out and impose this view of the world on others. If we follow Eric Jones's argument, the European lead in technology was a function of the political system first and, behind that, a rather exceptional resource base and set of ecological circumstances.[47] The major difference between Europe and the rest of the world at about the time that Europe began to enslave others was the diffusion of political power, both between and within European states. According to the early version of Jones's thesis, this diffusion of power curtailed wrong-headed decision making by centralized authorities and encouraged competition between states. It also allowed economies of scale arising from the free movement of resources and the maintenance of a shared market and knowledge pool large enough to foster the possibility of a slowly rising per capita income. This tendency was further encouraged by the early evolution of the nuclear family structure and a strategy of stressing goods over children, both of which were rooted in environmental characteristics peculiar to Europe.

In the long run, these social and economic patterns were hostile to enslavement of Europeans, and eventually to all slavery, in two ways. One was their tendency to stress the value of the individual, at least compared to the non-European world. The system of competitive nation-states in an overall balance of power relationship with each other was complemented within each state by an implicit and

[46] Stanley L. Engerman, "Coerced and Free Labor: Property Rights and the Development of the Labor Force," *Explorations in Economic History*, 29 (1992): 3.

[47] Eric L. Jones, *The European Miracle: Environments, Economies, and Geopolitics in the History of Europe and Asia* (Cambridge, 1981). The major criticisms of this work, which the author has attempted to address in subsequent publications, hinge on questions of individual motivation and the universality of "economic man" rather than his description of how Europe differed from the rest of the world.

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unusual contract between ruler and ruled. In return, for a large share of the peasants' small productive surplus, rulers suppressed random violence and provided stable legal institutions. The potential for individual rights was altogether greater in such a system than in a highly centralized empire or in regions where decentralized political power was associated with very low population densities. Conceptions of the individual as owner of him or herself, owing nothing to society, and of society as a series of "relations of exchange between proprietors," required restraints on political power as well as the encouragement provided by an evolving market society.[48]

A second and perhaps more important implication of the comparative patterns that Jones describes was the opportunity they provided for the pervasive growth of market behavior. The trading of land, labor, and capital, as well as goods and services, was far more extensive in early modern Europe than in any other continent. The ramifications for slavery of an extensive market system are ambivalent and not easily summarized in an essay. Slavery was the commodification of human capital carried to extremes and might reinforce as well as draw strength from the spread of markets. But, in a series of controversial essays on the link between capitalism and abolition, Thomas Haskell has argued that, under certain conditions, markets work against slavery and the infliction of cruelty in general. Haskell has listed preconditions for the historical emergence of humanitarianism as, first, an ethical maxim that makes the alleviation of suffering "right," second, a sense of being causally involved in the situation that gives rise to the suffering, and, lastly, possession of a recipe for intervention so easily applied that refusal to employ it might be considered "an intentional act in itself." The market system peculiar to the late eighteenth-century North Atlantic world "expanded the range of causal perception and inspired people's confidence in their power to intervene." The growth and elaboration of this knowledge incorporates, but is much broader than, technical and scientific learning. For Haskell, it is the link between causal perception and moral responsibility that is important. By the late eighteenth century, "recipe knowledge and causal perception," specifically one assumes in long-distance trade, had become so familiar that in the minds of some participants in the North Atlantic economy—notably Quakers—action against slavery (and other abuses) had become a moral imperative.[49]

What interests us here, however, are not the implications of the above argument for the abolition of slavery but its implications for the origins of the system. Before 1750, these same traits, again as embodied in market behavior, could have had almost exactly the opposite effect. Until the "recipe knowledge" had become so familiar that not to act became "an intentional act in itself," such

[48] C. B. Macpherson, *The Political Theory of Possessive Individualism: Hobbes to Locke* (Oxford, 1962), esp. 46–70. The quote is from page 3. For more specific implications of possessive individualism for free and coerced labor, see Engerman, "Coerced and Free Labor," 1–29.

[49] Thomas L. Haskell, "Capitalism and the Origins of the Humanitarian Sensibility, Part 1," and "Capitalism and the Origins of the Humanitarian Sensibility, Part 2," in Thomas Bender, ed., *The Antislavery Debate: Capitalism and Abolitionism as a Problem in Historical Interpretation* (Berkeley, Calif., 1992), 107–60, esp. 129–33 and 147–51. These essays were responses to the work of David Brion Davis and first appeared in the *American Historical Review* in April–June 1985.

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knowledge would serve to increase its possessor's power to intervene in the lives of others without generating any feeling of moral responsibility for the suffering that might result. Causal perceptions would lead to moral restrictions on enslaving one's weaker and poorer neighbor (or more broadly, perhaps, one's fellow European) but not to the purchase of a non-European who was already a slave in the Mediterranean, Africa, Asia, or the Americas. The organizational and technical skills of business, and the ability to develop long-term projects around scientific knowledge and bring them to fruition, could thus disrupt or destroy societies in distant lands without eliciting anything other than indifference on the part of the perpetrators. Dutch slaving activities in the East Indies in the early seventeenth century and English and Dutch use of Africans in the Americas had a larger impact on Asia and Africa than their Iberian counterparts of the previous century but generated no more self-questioning.[50]

Familiarity with "recipe knowledge" would first cross the threshold of action in the domestic rather than the colonial environment and be applied to "insiders" before "outsiders." An increasing regard for the consequences of one's actions at home was thus not inconsistent with continued indifference toward, and indeed encouragement of, slavery overseas.[51] Moreover, countries in which the market was more pervasive, such as the Netherlands and England, might tend to have greater respect for individual rights and more humane treatment of convicts, for instance, than would other countries with a slightly less invasive experience of market institutions. By the mid-seventeenth century, it was widely believed in England, the Netherlands, and France that no one entering those countries could remain a slave. Curiously, it was thought in England that this had been the case since the advent of Christianity.[52] As noted above, the French altered their laws so that those returning from the Caribbean could bring personal slaves with them, but there was no such enabling legislation in the Netherlands or England. It is therefore ironic and yet not surprising that the English and the Dutch developed the most absolute form of chattel slavery and the harshest laws against free blacks in the Americas.

The ubiquity of the market and recognition of individual rights were characteristics of all of early modern Western Europe viewed from a global perspective.

[50] J. Fox, "'For Good and Sufficient Reasons': An Examination of Early Dutch East India Company Ordinances on Slaves and Slavery," Anthony Reid, ed., *Slavery, Bondage, and Dependency in South-East Asia* (St. Lucia, Queensland, 1983), 246–62. Fox observes, "The Dutch East India Company embodied new organizational principles whose introduction into Asia had profound effects . . . the Company was the first formal slave holding corporation of its kind in Asia" (p. 248). Together with their English counterparts, the Dutch West India Companies probably doubled the volume of the transatlantic slave trade in the mid-seventeenth century.

[51] Abolition of colonial slavery may have been the first of the humanitarian reforms of the modern era, but the basic rights of individuals under the law in northwestern Europe were established before the creation of transatlantic European slave colonies.

[52] A handbook on England partly written for and widely used by foreigners was Edward Chamberlayne, *Angliae Notitia, or, The Present State of England . . . : Together with Divers Reflections upon the Antient State Thereof*, 6th edn. (London, 1672). It stated, "Foreign slaves in England are none, since Christianity prevailed. A foreign Slave brought into England, is upon landing ipso facto free from slavery, but not from ordinary service" (p. 331). See also Dudley North, *Observations and Advices Œconomical* (London, 1669), 45. For the status of blacks in England prior to the Somerset case (late eighteenth century), see Seymour Drescher, *Capitalism and Antislavery: British Mobilization in Comparative Perspective* (London, 1986), 25–49.

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But these conditions evolved furthest in the Netherlands and England. Three interrelated characteristics of agriculture and family life in those countries supported such conceptions of the individual. (All were driven by and in turn reinforced market penetration of the two societies.) One was the early emergence of the nuclear family and the associated phenomena of late marriage and early entry of children into service. A second was the non-feudal nature of land ownership, which facilitated the emergence of a well-integrated market for labor. A third was the related advancements in food production that permitted the support of a larger non-agricultural population.[53] These were the foundations of relative political and economic freedom (not to be confused with freedom from want) and limits on the reduction of Europeans to slave or near-slave status.

Beyond these common factors, there were important differences. In England, the notion that the privileges of the individual are ultimately of greater importance than those of the state or any group was probably more highly developed than anywhere in Europe.[54] Dutch attitudes derived not so much from concern for the individual as from a sense of the fragility of the social compact at a time when the country contained many resident non-Dutch and non-Calvinists. The greater prosperity of the Netherlands attracted large numbers of immigrants, but, despite illiberal edicts issued at the behest of the Calvinist church, the Dutch developed a practical tolerance of others unrivaled in seventeenth-century Europe.[55] It would be tempting to see in this a capacity for absorbing "outsiders" in the sense used above, but very few of these migrants were in fact non-Europeans. Far more non-Europeans lived in England, where their legal status was clarified only in the late eighteenth century.[56]

The foregoing paragraph suggests the impossibility of using convicts as a source of permanent colonial labor in England or the Netherlands. Penal transportation did not exist among the Dutch. In the case of England, scholars have until recently disagreed in their interpretations of criminal sanctions. They all nowadays seem to accept the view that judges and juries were reluctant to apply the letter of the more ferocious laws, even though scholars are not yet in accord on the reasons. If, for example, judges and juries were slow to enforce branding on the cheek for felons at the outset of the eighteenth century, it is doubtful that they would have sent convicts to chattel slavery in the Americas. If transportation

[53] Alan Macfarlane, *The Origins of English Individualism: The Family, Property and Social Transition* (Oxford, 1978); Jan De Vries, *The Dutch Rural Economy in the Golden Age, 1500–1700* (New Haven, Conn., 1974), 107–73; De Vries, *European Urbanization, 1500–1800* (Cambridge, Mass., 1984), 116–18, 210–12; G. E. Fussell, "Low Countries' Influence on English Farming," *English Historical Review*, 74 (October 1959): 611–22. By the end of the eighteenth century, agricultural output per hectare in the Netherlands and England were much the highest in Europe; Paul Bairoch, "Les trois révolutions agricoles du monde développe: Rendements et productivité de 1800 à 1985," *Annales: Economies, Sociétés, Civilisations*, 44 (March–April 1989): 318.

[54] Macpherson, *Possessive Individualism*, 263–71. See Macfarlane, *Origins of English Individualism*, 165–88, on the differences between early modern England and Continental Europe. Criticisms of his attempt to carry the comparison back into medieval times have tended to dominate evaluations of this book.

[55] E. H. Kossmann, "Freedom in Seventeenth-Century Dutch Thought and Practice," Jonathan I. Israel, ed., *The Anglo-Dutch Moment: Essays on the Glorious Revolution and Its World Impact* (Cambridge, 1991), 281–98.

[56] Drescher, *Capitalism and Antislavery*, 25–49.

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had meant a lifetime of servitude for English people, juries might not have convicted anyone at all.[57]

This reluctance may be understood as a function of shared community values or of fears about the resistance that would have inevitably followed. But what seems incontestable is that, in regard to slavery, the sense of the appropriate was shared across social divisions and cannot easily be explained by ideological differences or power relationships between classes. Outrage at the treatment of Africans was rarely expressed at any level of society before the late eighteenth century. The moral economy of the English crowd and the various Christian churches were preoccupied with other issues. When the immorality of coerced labor was finally recognized, the recognition appeared among all social groups at about the same time. And attempts to account for the failure of Europeans to enslave their own in terms of solidarity among the potential slaves do not seem promising.[58] If the elite could kill Irish, Huguenots, Jews, prisoners of war, convicts, and many other marginalized groups, why could they not enslave them? The English considered those from the Celtic fringe very different from themselves but not different enough to enslave.[59] For elite and non-elite alike, enslavement remained a fate for which only non-Europeans were qualified.

The central development shaping Western plantation slavery from the sixteenth century onward was the extension of European attitudes to the non-European world. If, by the sixteenth century, it had become unacceptable for Europeans to enslave other Europeans, by the end of the nineteenth century, it was unacceptable to enslave anyone. Put in relative perspective, before the eighteenth century, Europeans, in common with most peoples in the world, were unable to include those beyond the oceans in their conception of the social contract.[60] Unlike most other peoples in the world, Europeans had the power to impose their own version of that contract on others, which for three centuries meant African slavery.[61]

[57] See, for example, Beattie, *Crime and the Courts in England*, 450–519, esp. 490–92; Peter Linebaugh, *The London Hanged: Crime and Civil Society in the Eighteenth Century* (Cambridge, 1992).

[58] "Popular revulsion to bondage and untrammelled private power," which had "long preceded . . . critiques of colonial slavery," is one of three sources of anti-slavery sentiment singled out by Blackburn, *Overthrow of Colonial Slavery*, 36. He and Drescher, *Capitalism and Antislavery*, see popular involvement as vital to abolition. Like the churches, the intellectuals, and the political elite, however, those without property clearly had other priorities before the later eighteenth century. Attempts to enslave Europeans and put them to work on sugar plantations would perhaps have attracted their attention, but were plantation owners, metropolitan merchants, and mercantilist statesmen so apprehensive of popular opinion that not a single one of them dared advocate European slavery in public? A more convincing explanation of the silence, and inaction, is simply a conception, shared by all classes, of what was appropriate.

[59] George M. Fredrickson, *White Supremacy: A Comparative Study in American and South African History* (New York, 1981), 15.

[60] Drescher, *Capitalism and Antislavery*, 1–24. There is a striking complementarity between the shift in English perceptions outlined by Drescher and the market-driven emergence of humanitarianism that Haskell argues for in "Capitalism and the Origins of the Humanitarian Sensibility."

[61] A somewhat attenuated version of the same process might be seen in Japan, the non-European society with the most "Western" family and social structure before the twentieth century. Slaves in Japan were overwhelmingly Japanese—drawn from criminals and the poor—before the modern period; Patterson, *Slavery and Social Death*, 127. In the nineteenth and twentieth centuries, the Japanese increasingly drew on foreign sources for slaves, although the numbers were much lower than those carried across the Atlantic. Both before and after Japan imposed the abolition of slavery on Korea in 1910, Korea had come to be the major source of forced laborers taken to Japan. The

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The first step toward abolition of slavery generally may have been the idea that the enslavement of Europeans anywhere was a wrong that needed to be righted. In sixteenth-century Spain and Portugal, religious orders dedicated to the redemption of Spanish and Portuguese captives held as slaves in North Africa raised huge sums privately for that purpose, and the state was heavily involved by the end of the century. This activity and the funds behind it were far more significant than similar efforts made during the Middle Ages. They were much greater, too, than equivalent efforts in North Africa to redeem Muslim captives in Iberia; Muslim exchanges for Christians did not begin on a large scale until after 1750.[62] Farther north, almost every coastal town in the Netherlands had a "slave fund" for redeeming Dutch sailors from the galleys of the Barbary States.[63] European seafaring states signed a series of treaties with North African powers and the Ottoman Turks to safeguard ships and crews from capture and enslavement. Most involved the issuing of safe-conduct passes to merchant ships.[64] The irony that some of the main beneficiaries of such arrangements were Dutch and English slave traders on their way to Africa appears to have escaped historians and contemporaries, among them the earl of Inchquin, who was once held captive in Algiers before becoming governor of Jamaica. When the passes proved ineffective and seamen were captured and enslaved anyway, petitions to the British government seeking their release demanded action in the cause of "Christian charity and humanity"—long before abolitionists began to invoke similar principles.[65] Indeed, the Lady Mico Trust, used in the nineteenth century to fund education in the British West Indies in the aftermath of slavery, was first established in 1670 to redeem Christians from North Africa. Like Samuel Pepys and presumably the earl of Inchquin, the early Mico trustees could see the suffering of Europeans in the Barbary States but not of Africans in America.[66] The relevant question is, at what point did Europeans in large enough numbers to make a difference begin to understand that "Christian charity and humanity" should also encompass those of African descent?

The gradual removal of the barriers that kept non-Europeans from insider status was a very slow process and in some respects has never been completed.[67]

absorption of foreigners into the Japanese view of the social contract—a process that some would argue is not yet complete—might account for the gradual disappearance of coercion of foreigners, even though in this case there are extraneous geo-political developments to be reckoned with in the form of world wars. Mikiso Hane, *Peasants, Rebels, and Outcasts: The Underside of Modern Japan* (New York, 1982), 236–37.

[62] Ellen G. Friedman, *Spanish Captives in North Africa in the Early Modern Age* (Madison, Wis., 1983), 105–28, 157.

[63] Personal communication from Pieter C. Emmer, December 8, 1992.

[64] David Richardson, *The Mediterranean Passes in the Public Records Office, London* (East Ardsley, 1981). Among the first duties of ships commissioned by the early American Republic at the end of the eighteenth century was protection of U.S. shipping against the Barbary pirates.

[65] PRO, James Kirkwood to Secretary of State, June 6, 1709, CO 388/12, K25.

[66] Frank Cundall, *The Mico College, Jamaica* (Kingston, 1914), 7–9. Samuel Pepys' comments on the European slaves held by the Barbary States are in his diaries dated January 8 and November 28, 1661.

[67] Philip D. Morgan argues that white attitudes toward blacks hardened during the colonial period. He does not take up the emancipation issue, which arose in the northern states during and after the revolution, or attitudes toward blacks in the abolition movement in the wider Atlantic region during the eighteenth century. "British Encounters with Africans and African-Americans, circa 1600–1780,"

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As noted above, it was extended to Native Americans before Africans and perhaps to non-Europeans living in Europe before their overseas counterparts. The widening of this insider status can be traced in intellectual developments. While no major thinker after Locke was able to defend the practice of enslaving those outside the social contract, the ambivalence of the Enlightenment on the issue of slavery for non-Europeans is evident.[68] It can also be traced at the level of personal experience in both the Anglo-Saxon and the Hispanic Atlantic, where proximity to the exploitation could, for a minority, breed sensitivity as well as indifference.[69] It is possible that among the European working class, the first abolitionists, or at least those who were prepared to identify with blacks, were seamen, rather than industrial artisans.[70] Certainly, the European country with the largest and most successful slave trade and slave colonies in both relative and absolute terms was the center of the strongest abolition movement and the leader of world abolitionism. British subjects, moreover, had an extraordinary proclivity to migrate, most of them before 1800 going to plantation regions that formed the most integrated of all eighteenth-century colonial systems. As early as 1700, "communication and community" across the British Atlantic had attained a depth, richness, and reliability of contact unrivaled among European powers and unprecedented in the history of long-distance migration.[71] Such characteristics have clear implications for metropolitan awareness of events "beyond the line."

The Netherlands, by contrast, despite its early commitment to social and economic freedom, was among the last countries to take action and did not experience a mass antislavery movement of any kind. Also by contrast, it was without a slave-trade fleet after the 1780s, and the slave sectors of its empire were not only small but of declining significance from the late eighteenth century. More important, in the two centuries before 1800, the Dutch experienced net immigration of half a million people, almost all of them from other parts of northwestern Europe. Perhaps a mere 30,000 people left the Netherlands for the Americas and Africa on a net basis, and many of these were not Dutch. The English over the same period had a net emigration of 1.25 million, almost all of whom settled around the Atlantic basin. Dutch transatlantic networks and contacts with non-Europeans were simply many times less dense and frequent. Even if we include the Dutch East Indies, communication between England and

in Bernard Bailyn and Philip D. Morgan, eds., *Strangers within the Realm: Cultural Margins of the First British Empire* (Chapel Hill, N.C., 1991), 157–219.

[68] Davis, *Problem of Slavery in Western Culture*, 137–39, 319–479; Davis, *Slavery and Human Progress*, 107–16, 154–58. Also see the discussion of Montesquieu in Seymour Drescher, "Capitalism and Abolition: Values and Forces in Britain, 1783–1814," in Roger Anstey and P. E. H. Hair, eds., *Liverpool, the African Slave Trade, and Abolition* (Liverpool, 1976), 182. It would seem that for Locke the universe of the social contract was not the state but rather the European nation-state.

[69] John Newton and Alexander Falconbridge are among the better-known slave ship officers who turned against the trade.

[70] Peter Linebaugh and Marcus Rediker, "The Many Headed Hydra: Sailors, Slaves and the Atlantic Working Class in the Eighteenth Century," in Colin Howell and Richard J. Twomey, eds., *Jack Tar in History: Essays in the History of Maritime Life and Labour* (Fredericton, N.B., 1991), 11–36. For the wide range of attitudes of Europeans and Africans toward each other, see Morgan, "British Encounters with Africans and African-Americans," 157–219.

[71] Ian K. Steele, *The English Atlantic, 1675–1740: An Exploration of Communication and Community* (New York, 1986).

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the transoceanic world—and, accordingly, daily awareness of the world outside Europe—must have dwarfed that of the Netherlands.[72] This is not, however, the place to reassess the origins of abolitionism. My intention is to suggest only that the key counterpoint is not slavery and abolition but rather the enslavement of non-Europeans and abolition.[73]

THE ABOVE ARGUMENTS have some further implications. One is for the well-known debate on the origin of white attitudes toward black people. As previously noted, the evidence presented here points to major non-economic factors in the decision to use African slaves. While it certainly became profitable to replace European indentured servants with African slaves, the main issue was not relative profits but rather the inability of colonists to conceive of Europeans as chattel slaves. Such a conception might well have developed given enough time, but the sugar revolution proceeded too quickly to allow Europeans to adjust perceptions of insiders and outsiders. If Europeans had been able to accord Africans the same rights as themselves in the early modern period, they would not have enslaved them and brought them to the plantation Americas. The absence of European slaves, like the dog that did not bark, is perhaps the clue to understanding the slave trade and the system it supported.

A second implication, less obvious but just as important, is that Europeans, and more particularly the English, failed to take advantage of two significant economic opportunities. For the first, if they had emulated the sixteenth-century Russian aristocracy by creating an ideological distance between the common people and themselves and enslaving some members of their own society, they would have enjoyed lower labor costs, a faster development of the Americas, and higher exports and income levels on both sides of the Atlantic. For those who see European, especially English, economic power built on overseas colonies, it might be argued that, for the underpopulated tropical Americas at least, exploitation of the periphery and the transfer of surplus to the core would have been far more rapid with white slave labor. The second failure to maximize an economic advantage occurred when Europeans gradually widened their perception of what constituted an insider by beginning in the late eighteenth century to include transoceanic peoples. This change in perception brought a very profitable institution to an end. The first "missed opportunity" created the Atlantic slave

[72] Jan Lucassen, *Dutch Long Distance Migration: A Concise History, 1600–1900* (International Institute for Social History, IISH research paper, Amsterdam, 1991), 20–23, 35–42; and E. A. Wrigley and R. S. Schofield, *The Population History of England, 1541–1871: A Reconstruction* (London, 1981), 528–29. Probably fewer than 1,000 Dutch individuals a year returned from the East Indies, most of them sailors and soldiers. Most of those who did not return died (Lucassen, *Dutch Long Distance Migration*, 41). For the New Netherlands, Seymour Drescher points to a lack of support networks and leaders who wished to reproduce Dutch culture in the process of pursuing the issue of why the Dutch were such tardy abolitionists; Seymour Drescher, "The Long Goodbye: Dutch Capitalism and Antislavery in Comparative Perspective," forthcoming, *AHR*, February 1994.

[73] For the recent literature and a judicious discussion of the roots of antislavery in the English context, see David Turley, *The Culture of English Antislavery, 1780–1860* (London, 1991), 1–46, 227–36. For the decline of Dutch slavery in the East Indies, where it might be noted that the institution was not related to export production, see Susan Abeyasekere, "Slaves in Batavia: Insights from a Slave Register," in Reid, *Slavery, Bondage, and Dependency*, 286–314, esp. 308–10.

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trade from Africa; the second ended not only the slave trade but slavery in the Americas as well.

A third implication—really an extension of the previous point—consists of the notion that seventeenth-century capitalism, mercantile or not, was hardly as unrestrained and voracious as many students of early modern Europe have portrayed it. Profit-maximizing behavior occurred within agreed-upon limits, limits defined at least as much by shared values as by the resistance of the less-propertied classes. This should not be surprising. There have always been extensive areas of personal life defined as beyond the limits of the market and profit maximization even in the Western world, and the expansion and contraction of such an area is a major field of study, particularly as it pertains to wage labor. By the seventeenth century, enslavement of fellow Europeans was beyond the limits. More interesting, however, is that the peoples with the most advanced capitalist culture, the Dutch and the English, were also the Europeans least likely to subject their own citizens to forced labor.

The fourth implication is the corollary of this last point. The countries least likely to enslave their own had the harshest and most sophisticated system of exploiting enslaved non-Europeans. Overall, the English and Dutch conception of the role of the individual in metropolitan society ensured the accelerated development of African chattel slavery in the Americas (and Asian slavery in the East Indies) because their own subjects could not become chattel slaves or even convicts for life. Further implications touch the English Revolution as well as its American counterpart of nearly a century later. There may be something to be said for expanding a variation of Edmund Morgan's argument to cover the whole of the British Atlantic, in the sense that the celebration of British liberties—more specifically, liberties for Englishmen—depended on African slavery. But if this route leads to an interpretation of history driven purely by whichever interest group was dominant, then we should pause. Capitalists could have made more by selling European convicts for life than African slaves, and more again if the progeny of those convicts, like Africans, had also been bound to a lifetime of service. In the end, the absence of European slavery suggests that narrowly economic interpretations of history often miss the point.

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Construing the Harvest: Gleaners, Farmers, and Officials in Early Modern France

LIANA VARDI

THE STORY ENDS in the middle of the nineteenth century. It culminates in a famous artistic representation, one that has molded our perceptions ever since. Jean-François Millet's painting of women gleaning in a field first appeared at the Paris salon of 1857. Its success was not immediate. Its subsequent career is therefore the more instructive. By the end of the century, this particular painting, like its companion piece by the same artist, *The Angelus*, had become one of the most popular and most reproduced scenes of French life. To this day, every Parisian secondhand postcard shop carries innumerable turn-of-the-century depictions of these works. Depiction rather than copy is the appropriate term here. The original often gave rise to fanciful variations that distorted color and detail, even if the basic features were respected. This was not a simple process of reproduction but a continuous and creative interaction with familiar and beloved themes. The pictures became so emblematic, in fact, that propagandists during World War I used them to illustrate the values of home and work for which French soldiers were laying down their lives.

What does *The Gleaners* show? On a barren and rust-colored foreground, three women bend down, gathering a few dry stalks. Their movements are heavy but not graceless, and they themselves display the stereotypical thick-set features that dominate so many northern representations of peasants. They embody an animal force deeply absorbed by a painstaking task. Their gaze never meets the viewer's. It is directed at the meager pickings before them.

In the background, an abundant crop has been gathered. Golden towers of wheat rise behind the gleaners, and an indistinct group of harvesters is busy bundling, stacking, and carting the sheaves. On the right, a single figure on horseback, the landowner or his steward, observes the scene. The contrast between wealth and poverty, power and helplessness, male and female spheres is forcefully rendered. This was intentional. In earlier versions of the painting, Millet had placed the women next to the harvesters.[1] Their removal emphasized

The research for this article was carried out thanks to a grant from the Social Sciences and Humanities Research Council of Canada. Versions of this work were presented at the Conference in Honor of Leo Loubère, Buffalo, New York, March 1992, and at the meeting of the Society for French Historical Studies, El Paso, Texas, March 1992. I would like to thank the audience and commentators for their suggestions, as well as the reviewers of this article and Jonathan Dewald.

[1] Robert Herbert in *Millet's Gleaners*, The Minneapolis Institute of Arts Exhibition Catalogue (Minneapolis, Minn., 1978), 16–18.

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Jean-François Millet, "Les glaneuses" [The Gleaners], 1857. The Orsay, © Reunion des Musées Nationaux, Paris.

the privation and impotence that Millet fatalistically ascribed to the peasants.[2] His contemporaries responded in kind. Some denounced while others lauded this portrayal of rural life. For the art critic of *Le figaro*, it was a call to revolution, summoning up "the pikes of '93." Others, like the reviewer for *La presse*, resented the glorification of misery.[3] A more sympathetic critic tried to reassure the public: "This canvas, which calls up frightful miseries, is not like some of Courbet's pictures, a political harangue or a social tract. It is a very beautiful and very simple work of art, free of all declamation."[4] The picture was gauged less on its artistic merits than on the message it conveyed. Whatever Millet's own iconographic references and intentions may have been, viewers reacted to the naturalism of the scene. The bleakness of the gleaners' lot seemed to cry out for redress or else appeared to be a dangerous reminder of social inequities. On the whole, the veracity of what was portrayed was not seriously challenged. The painting may have been considered vulgar or inappropriate, and Millet's usual detractors may have once more been appalled by his subjects' numbed, brutish expressions, but

[2] Alfred Sensier, *Jean-François Millet, Peasant and Painter*, Helena De Kay, trans. (Boston, 1881), 126.
[3] Robert L. Herbert, *Jean-François Millet* (Paris, 1975), 101.
[4] Jules Castagnary reporting on the Salon of 1857; see Elizabeth Gilmore Holt, *The Art of All Nations: 1850–73; The Emerging Role of Exhibitions and Critics* (Garden City, N.Y., 1981), 204–05.

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for most elite viewers he had shown peasant life, whether it was a worthy artistic subject or not.

Millet's real fame came after his death, with a major retrospective of his work in 1885. By then, the agricultural crisis had created a massive exodus from rural areas that once again highlighted the peasants' miserable lot. Yet the crisis reduced the concentration of population and therefore the fears of a potentially disgruntled or revolutionary force in the countryside. It became easier to attach such threats exclusively to the urban proletariat and consequently to sentimentalize rural poverty. Millet's figures thus became imbued with a nobility, piety, and passivity that struck an overwhelmingly sympathetic chord.[5]

Millet's *Gleaners* expressed the rural world as late nineteenth-century urban viewers had come to understand it. These perceptions inflect our own. We presume the realism of the scene and accept its social commentary without finding it threatening. The women before us are gleaners, doing what they can to feed themselves, doing what gleaners did. Our vision of gleaning is the vision put forward by Millet. Poor women come to gather what little remains on the field after the harvest. This right has belonged to them from time immemorial.

GLEANING HAS APPEARED so straightforward an issue that it has received little detailed attention. The subject inspired a single French dissertation, in law, in the early years of the twentieth century.[6] Research remains sparse, although one English scholar has recently attempted to calculate both its worth and its place in the household economy.[7] Ignorance of the actual practice of gleaning has not meant silence about it in the historical literature. Quite the contrary: gleaning appears in many a rural study, because it seems clearly to epitomize villagers' communal rights and the limitations that the Old Regime placed on private property.[8] The land's produce did not belong entirely to the farmer. Much is thus at stake in the history of gleaning: the nature of communal life, agricultural methods, labor relations within the village, approaches to property, changing perceptions of charity, as well as definitions of criminality and marginality. Gleaning also raises questions about the role of women in the rural economy, because historians have tended to view it as an instance of the sexual division of labor. Yet the framework in which gleaning has actually been examined has remained overly simple.

The standard interpretation is exemplified in the work of E. P. Thompson and his students.[9] Although Thompson deals with England, his assumptions about

[5] See, for example, André Fermigier, *Jean-François Millet* (Geneva, 1977), 16.

[6] Paul Degrully, *Le droit de glanage, grappillage, râtelage, chaumage et sarclage: Patrimoine des pauvres* (Montpellier, 1912).

[7] Peter King, "Gleaners, Farmers and the Failure of Legal Sanctions in England 1750–1850," *Past and Present*, 125 (November 1989): 116–50; and King, "Customary Rights and Women's Earnings: The Importance of Gleaning to the Rural Labouring Poor, 1750–1850," *Economic History Review*, 44 (1991): 461–76.

[8] This is especially true for England. See, for example, K. D. M. Snell, *Annals of the Labouring Poor: Social Change and Agrarian England, 1660–1900* (Cambridge, 1985).

[9] See, for example, the recent restatement in E. P. Thompson, "Custom, Law and Common Right," in his *Customs in Common* (New York, 1991), 97–184.

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rural life are apparent in the literature on France. I have chosen Thompson as the primary example of this approach because he makes explicit what French historians usually take for granted. The interpretation, then, runs approximately as follows. Gleaning was one of those subsidiary rights to local resources, such as woodland or pastures, which had devolved to the peasantry from time immemorial, and to which they were firmly attached because these rights provided part of their livelihood. Depending on their uses and interpretation, these communal rights could nonetheless serve a number of interests, from those of the lord and his tenant-farmers to those of the poorest squatter. With the development of a market economy, these customary rights came to interfere with newly individualistic notions of property and insistent demands for productivity. In the eighteenth century, conflicts over gleaning rights became more and more common. Profit-oriented farmers resented communal claims to the land and fought encroachments on their property, and the state supported their claims. No longer would the community freely pasture animals, or collect the leftovers of the harvest, or scavenge in the forests. The losers, in this interpretation, were the village poor, who had relied on such supplements. In these circumstances, the rural community became destabilized and divided. Those able to profit from agrarian capitalism asserted their prerogatives to the detriment of the rest. In previous times, the community had operated under the assumption that the local poor were entitled to subsistence. Communal rights had embodied claims by the poor to share in the village's bounty. They had also enshrined the community's sense of responsibility for everyone's welfare. These practices were seriously challenged by a different set of assumptions that stressed individual profit and individual property. The poor were left to fend for themselves.

In the interpretation summarized above, the attack on gleaning encapsulates the ruthlessness of a new capitalist ideology, for this assault affected the truly indigent, those most deserving of care. The right to pick the remains of the harvest had essentially been theirs and had secured their livelihood over the winter. Now farmers were eager to collect the leftovers themselves and to keep out intruders. For Thompson, capitalism was dissolving ancient commitments and responsibilities. The poor no longer had automatic access to the village's resources. Moreover, the attack was especially heartless because gleaning is usually considered a female prerogative.[10] In a recent volume on the history of women in the early modern period, for example, we read that the duties of the farmer's wife included "tending livestock, growing vegetables, keeping bees, sewing, mending, preserving, lending a hand at the harvest and exploiting the family's gleaning rights in the community."[11] Peter King states that "gleaning was one of the few customary activities controlled almost exclusively by women."[12] Capitalist devel-

[10] If gleaning was a female activity, of the sort suggested by Millet, then we remain as ignorant about it as about other areas under female control like the chicken coop or barnyard. But we know that gleaning was not the exclusive domain of women and that, far more than supplemental kitchen produce, it involved agricultural production as a whole, for it was an integral part of the harvest.

[11] Olwen Hufton, "Le travail et la famille," in Arlette Farge and Natalie Zemon Davis, eds., *Histoire des femmes en Occident*, vol. 3: *XVI^e–XVIII^e siècles* (Paris, 1991), 42. Original English text given in *The New York Review of Books*, December 19, 1991, 68.

[12] King, "Customary Rights," 462.

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opment, in this interpretation, meant that the poor widow was denied her gleanings.[13]

This article will look at gleaning practices in France from the Middle Ages through the eighteenth century and will argue that the interpretations mentioned above misrepresent its nature. Gleaning did not embody a traditional world, one of mutual responsibility, that was being destroyed. Gleaning, although a long-standing practice, had not always been reserved for the poor and certainly not for poor women. Rather, it had been treated as the last phase of the harvest, involving a complex range of labor arrangements. In sixteenth-century France, a more interventionist central government, obsessed with rural taxation, increasingly meddled with the process. Gleaning was separated from harvesting in ways, it was thought, that would secure the crops from potential theft. In doing so, the authorities nurtured (and in some places awakened) a sense of invasion, trespass, and thus of individual property. The gleaning laws themselves rather than a new economic calculus were instrumental in fomenting conflict. Instead of pitting farmers against laborers, more often than not the legislation united farmers and laborers (some looking for profit, others for their livelihood) against the authorities.

Separating gleaning from harvesting (as the French state sought to do) disrupted agricultural practices. It also delineated a new category of poor, and the help they were to receive, within the rural community. This was part of a general movement in Renaissance Europe to define clearly a body of deserving poor.[14] Gleaning would thus serve as charity. This would be its essential purpose and would restore it to its original function. This theologically based, learned view of gleaning, drawn primarily from the Bible, supplied the moral underpinnings for many gleaning regulations and subsequent prosecutions. Gathering the fallen grain ceased to be a practical concern, a normal step in the harvest, and became imbued instead with moral imperatives. The poverty, marginality, and class conflict that Millet depicted in the mid-nineteenth century were a creation of the state, with the help of a religious rhetoric.

Our image of gleaning, then, is of recent vintage. It was constructed in the early modern period, although it borrowed from, and claimed to be the heir of, ancient practices. As a result, the chronological boundaries of this study will be fluid, extending occasionally to these earlier models. Legislation on gleaning became common in the sixteenth century, but most of our practical knowledge comes from the eighteenth century. In describing the practices themselves, I will use evidence from several regions of France: the north, south, center, and west. Not all areas will be represented: the pasturing highlands in the east, for example, showed no interest in the issue.[15] Some of my examples will involve small family

[13] King presents the standard view in his two articles, "Customary Rights," 470–71, and "Gleaners, Farmers," 116. He argues, however, that they were far more successful than previously believed in fending off this attack.

[14] See, for example, Paul Slack, *Poverty and Policy in Tudor and Stuart England* (London, 1988); and, for a recent French overview, Philippe Sassier, *Du bon usage des pauvres: Histoire d'un thème politique (XVI^e–XX^e siècle)* (Paris, 1990).

[15] I found no litigation on gleaning in seigneurial, royal, or ecclesiastical courts in Franche-Comté or Auvergne, where there were, by contrast, endless complaints concerning animal depredations, or *mésus*.

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farms, others large estates. Most of my examples about small properties will come from the south and those about big properties from the north, reflecting the contrast between the two Frances. Although historians have made much of regional differences, in this domain they are relatively unimportant. Gleaning was perceived as an invasion of private property by both small property owners and big tenant farmers, wherever they happened to reside. In all areas of France, farmers resented the state's interference with their labor arrangements, while agricultural workers tried to secure their earnings despite the state's insistence that gleaning rights go to a minority of paupers. The same basic conflicts recurred throughout France—partly because the state itself tended to impose national policies on gleaning.

GLEANING WAS AN ACTIVITY intimately bound up with the early modern harvest, because so much grain was lost while the crops were being cut. Should the stalk be slightly dry, or handled too brusquely, the grain would likely fall to the ground. This loss of grain was a primary concern of the farmer—indeed, according to one distinguished French historian, it was *the* central issue of the harvest and one that significantly affected agricultural technology.[16] To prevent such losses, the farmer had two options: to reap the crop early, when it had not fully ripened (but then it had to dry on the fields, where it could be stolen or damaged), or to use a sickle, which was a slower method of harvest but less destructive than the scythe.[17] Farmers continued to rely on the second option well into the nineteenth century, at least for the wheat crop, because it reduced the loss of grain.

The harvest involved five main stages. The first was the cutting of the stalks, the second the bundling into sheaves, the third the piling of the sheaves into mounds, the fourth the carting of the sheaves to the barn, where they would either be stored or threshed.[18] Lastly, the remains would be gathered. This could be done by the owner and his laborers, right after the bundling, as a specialized crew of rakers followed right behind the binders.[19] It could also be done after the harvest was stored. Gleaning by hand involved careful sifting and was therefore time consuming; it made more sense, where labor was scarce, to bring in the main part of the harvest before worrying about the leftovers. The fallen grain could also be left for the cattle grazing in the fields; or, again, it might be abandoned altogether and collected by those who needed it.[20]

[16] Jean Meuvret, *Le problème des subsistances à l'époque de Louis XIV*, 2 vols. (Paris, 1977), 1: 165. See also David Hoseason Morgan, *Harvesters and Harvesting, 1840–1900: A Study of the Rural Proletariat* (London, 1982), chap. 2.

[17] On the advantages of the different implements, see E. J. T. Collins, "Harvest Technology and Labour Supply in Britain, 1790–1870," *Economic History Review*, 22 (1969): 453–73; and Michael Roberts, "Sickles and Scythes: Women's Work and Men's Work at Harvest Time," *History Workshop*, no. 7 (1979): 3–28; for France, Meuvret, *Le problème des subsistances*, vol. 1, chap. 4.

[18] The grain was threshed immediately in the south and stored in the north, where it was threshed as the need arose, throughout the fall and winter.

[19] Note in this context that the Merriam Webster Third International dictionary uses the following citation to show how "gleaner" is used: "The gleaners must follow the reapers if full profit is to be realized—E. E. Pratt."

[20] This appears to have been the case in pasturing areas. The cattle grazed on the arable land after the grain harvest.

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How much grain fell to the ground? We are not told precisely. Amounts varied from year to year, depending on the weather, the state of the crops, and the nature of agricultural implements. The consensus, however, is that it was a significant proportion.[21] As long as the owners were entitled to collect it at their leisure and in the way that most suited them, the only problem was the allocation of labor. Until mechanization in the late nineteenth century, harvests were labor intensive. Crops had to be brought in very quickly. Since all fields ripened more or less simultaneously, all available hands had to be put to work, reaping and binding. Hiring might even extend to other villages or to groups of migrant workers.

The earliest surviving harvest regulations about gleaning, those in fourteenth-century English manorial by-laws or French local customaries, dealt precisely with these issues. The able-bodied were to hire themselves as reapers and binders and not as "gleaners," for this was an easier task that could be left to the weak and infirm. If one could be hired as a gleaner, gleaning must have been a remunerated activity, and one to which people would likely gravitate unless they were expressly forbidden from doing so.[22] The division of villagers into weak and strong, able-bodied and disabled, gleaners and non-gleaners, reflected the needs of the harvest, rather than philanthropic impulses. Gleaning was the sort of work best left to the young and aged or relegated to women once the crops had been stacked and the men were busy carting and threshing them.

Such gleaning might well have been done with rakes. In later periods, seventeenth-century England, for example, women rakers were regularly included among harvest crews.[23] They gathered the fallen grain for the farmers. In other words, they did the work that had previously been known as "gleaning." In England, then, the collection of the leftovers was split into two activities: raking and scavenging.[24] Since the farmer had already raked the fields, gleaning, in its restricted, post-medieval sense, we may conclude, did not reduce farm income in significant ways.

Until the sixteenth century, the situation in France was similar. During Roman times, although gleaning existed (known as *spicilegium*, from spikes, or ears of grain), the usual pattern was for the landowner and his workers to collect the remains as a normal part of the harvest, after which, cattle were let out to pasture.[25] The sixth-century Salic Laws mention gleaning, *glenare*, though per-

[21] In nineteenth-century England, contemporaries still assessed such losses as "terrible." Cited by Collins, "Harvest Technology," 462.

[22] This is what the evidence suggests for medieval England. See Warren O. Ault, *Open-Field Husbandry and the Village Community: A Study of Agrarian By-Laws in Medieval England* (Philadelphia, 1965), and "By-Laws of Gleaning and the Problem of the Harvest," *Economic History Review*, 14 (1961–62): 210–17. For France, Degrully cites the *coutumes* of Montreuil and Amiens, which forbade those who could earn 6 sous a day from gleaning; *Le droit de glanage*, 48.

[23] For example, Roberts, "Sickles and Scythes," 10, 23; and Morgan, *Harvesters and Harvesting*, 46.

[24] Descriptions of eighteenth and nineteenth-century gleaners confirm this. Unlike the French, who strictly limited gleaning periods when the "poor" had access to the fields, English women and children might spend several weeks scavenging. King, "Customary Rights," 474. King notes the change from the medieval to early modern period, in which gleaning becomes associated with the poor, without explaining the transition, 470.

[25] *La grande encyclopédie* (Paris, n.d.), 18: s. v. "Glanage," 1061, refers to Marcus Terentius Varro, *De re rustica*, Book 1, 53. See also K. D. White, *Roman Farming* (London, 1970), 183.

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haps in its earlier meaning of reaping.[26] "He who gleans another man's harvest without his consent shall be liable to pay six hundred denarii."[27] Then, centuries of silence.

In the later Middle Ages, the practice was to treat gleaning as an integral part of the harvest. Workers not needed elsewhere were assigned to gather the leftovers. Otherwise, the remains went to the cattle. If there were disagreements, they were primarily over the disposition of this grain. Some wanted to gather the fallen grain, whereas others wanted it left for their animals.[28] This conflict clearly entailed questions of livelihood, but the details remain murky. Whatever the case, communal pasturing rights openly clashed with gleaning rights, and efforts to pacify both sides engendered legislation strictly limiting the period of gleaning. Here, we see one set of communal rights undermining another, rather than capitalism whittling down communal practices.

The French state first intervened in the thirteenth century. Around 1260, Louis IX, king of France, issued an ordinance forbidding animals to enter the fields until three days after the end of the harvest, in order, as sixteenth-century commentators interpreted the law, to allow the poor to glean.[29] A regulation by the next king, Philippe III, in 1276, appears to read much the same way: it forbids pasturing for that same length of time, although without mentioning the poor.[30] The *coutume* of Etampes made similar provisions, allowing the gleaners twenty-four hours in the fields (once the crops had been removed) before the cattle were allowed to graze.[31] That of Péronne, in 1507, was more generous, granting gleaners three days.[32]

As early as the Middle Ages, then, gleaning was subject to regulations and restrictions. These revolved primarily around practical concerns, however: the allocation of work and the feeding of animals. As a system of three-course rotation came to dominate open-field regions, accompanied by communal grazing on the harvested fields, some peasants began to view gleaning as an inconvenience. Nonetheless, part of the fallen grain continued to be raked or gathered on behalf

[26] See Oscar Bloch and Walther von Wartburg, *Dictionnaire étymologique de la langue française* (Paris, 1968), 296; *Glossarium mediae et infimae latinitatis*, 3: 528; *Dictionnaire de l'ancienne langue française et de tous ses dialectes du IX^e^ au XVI^e^ siècle* (Abbeville, 1885), 4: 287, which show how the original meaning of the term changed. Degrully, however, merely cites all their examples as evidence of gleaning.

[27] Katherine Fischer Drew, trans., *The Laws of the Salian Franks* (Philadelphia, 1991), p. 154, capit. V, cxxvi.

[28] I found no gleaning regulations or gleaning cases in Auvergne or Franche-Comté: Archives Départementales (hereafter, A.D.) Doubs, Haute Loire, and Puy-de-Dôme. The only mention of gleaning comes from a village in Auvergne, where the local administrator requested that the animals be kept out of the harvested fields for twenty-four hours to allow the poor to collect the leftover wheat (*laisser aux pauvres le temps de ramasser les épis*). A.D. Puy-de-Dôme, 1F281, justice de Vodable, *ban de vendanges*, October 19, 1786.

[29] Isambert, Decrusy, and Armet, *Recueil général des anciennes lois françaises depuis l'an 420 jusqu'à la Révolution française* (Paris, 1828), 1: no. 229. The original has not survived but was reported by the legal commentator Jean Boutillier. We do not know, therefore, whether Louis was concerned with the poor, as Renaissance monarchs would later be. See Jean Boutillier, *Somme rurale très utile en toutes cours de praticqs* (*ca.* 1525).

[30] *Ordonnances des rois de France*, 1: 312.

[31] Article 190, cited in Joseph Nicolas Guyot, *Répertoire universel et raisonné de jurisprudence civile, criminelle, canonique et bénéficiale*, 17 vols. (Paris, 1784), 8: 161.

[32] Cited by Degrully, *Le droit de glanage*, 51. The same was true of the *coutumes* of Ponthieu, Amiens, Montreuil, the Boulonnais, and Artois.

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of the farmers, while the rest briefly became communal property. Raking was generally done by women, since it was physically less demanding and less well-paid.

A number of features generally attached to gleaning were therefore in place by the late Middle Ages. Gleaning had become a special activity, separated from the harvest proper, and often associated with women. It was also restricted to specific times. The reasons for this limitation were practical. Regulations were meant to ensure that some of the fallen grain remained available to the farmer and to the community, while the rest went to the cattle. Once communal grazing became the norm, everyone else logically felt entitled to gather some of the remains as well. Whatever the rationale, the result was a communal gleaning period (with or without previous raking) ranging from one to seven days, depending on the locality. The regulations did not concern themselves with who was entitled to glean.

RENAISSANCE STATES had a different outlook on gleaning, even if they repeated thirteenth-century injunctions. They manifested new interests in productivity and taxation and in the moral regeneration of the kingdom. Henry II of France issued the first general edict regulating gleaning for the entire country in 1554, during a period in which he was also endeavoring to control illegitimacy and infanticide and to rid the kingdom of ruffians. The edict described yet another scourge afflicting the kingdom, although it began by lauding the kindness of landowners who let everyone (*toutes personnes*) glean. Unfortunately, this privilege had been misused. Unscrupulous people in town and country stole the crops under the guise of gleaning.

> Likewise, since charity dictates that man serve his fellow man by showing himself generous, especially with what yields him no profit but could be useful to others, all the churchmen, nobles, burghers, farmers, and others who grow cereals, generously allow everyone, at harvest time, once the crops have been cut, to glean and take away the stalks left behind by the reapers. Yet We have learned that a number of scoundrels, both from the city suburbs and hinterland, make use of such permission and form gangs, and under the pretense of gleaning, steal the sheaves, wheat, and cereals left on the fields, both those that belong to the farmers and those left as payment for the tithes and other taxes. What is more, quite often, landowners and farmers, who grow wheat and cereals, evade the tithe by claiming that they left it on the fields and that the gleaners stole it.[33]

The king ordered all able-bodied males and females to hire themselves out as reapers and binders, for the appropriate wage, and to refrain from gleaning. Gleaning was confined to those such as the young or aged who were unable to perform these other tasks. They could gather the leftovers once the harvest had been stored and the tither and tax collector had taken their shares.[34] While the edict repeated some earlier concerns, these were overshadowed by a tense, negative rhetoric centered on greed, the greed of peasants who stole crops "under

[33] Isambert, *et al.*, *Recueil*, 13: 418–19. Previous royal ordinances had been restricted to specific parts of northern France.

[34] These injunctions were also repeated in the eighteenth century: "No man or woman who is able to do the harvest shall be allowed to glean." Arrêt of the bailliage court of Amiens, July 4, 1781, cited in Guyot, *Répertoire*, 8: 160.

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the pretense of gleaning" and later on the greed of landowners who denied the poor this form of agrarian charity. The legislation marginalized the economic significance of gleaning by defining it as something peripheral to the farmer, "that yields him no profit," that he could easily abandon to the poor. The state seems to have been genuinely unaware that the gleanings made up a significant percentage of the harvest.[35]

We can infer from Henry's edict that certain fears surrounded the practice of gleaning in the later Middle Ages. Apparently worried about losing their fair share, gleaners had begun gathering the remains right behind the reapers. This had not only reduced the farmer's chances of raking the fields to gather some of the fallen grain but it also put the crops at risk. The gleaners could easily grab some of the sheaves or standing grain. In the worst cases, the needy (described by Henry as bands of ruffians) converged on villages and stole the crops under the guise of gleaning. These threats appeared overwhelming to a government anxious to purge society of its disruptive elements. The fields would have to be protected from such hooligans. The new statutes therefore strengthened the gleaning prohibitions that had been elaborated by rural communities. Government intervention did not stop there. In the Middle Ages, localities had already named wardens to patrol the fields and ensure the orderly conduct of the harvest.[36] The crown intensified this rural policing by prompting villages to hire harvest guards, establishing a rural constabulary, and—with its gleaning and pasturing laws—limiting the number of people and beasts allowed on arable land at harvest time. Crops, farmers, and especially taxes would thus be safe.

Just as significant, the legislation of 1554 embodied the crown's new concern to safeguard all dues levied on the fields, especially the tithe, at a time when the crown had begun to rely on the church for a substantial part of its income. As of the sixteenth century, tithes were collected before the crops were stored away.[37] This increased the church's revenues by ensuring a truly proportional intake, typically every thirteenth sheaf. The church's income thus grew but at the same time became more vulnerable. The presence of crowds of scavengers was especially distressing. Farmers, fearing the rain and other damages, removed the crops as soon as they could, leaving the tithe behind on the open country, despite repeated prohibitions. This grain might then disappear into the aprons of the gleaners, or else the farmer might claim such losses and defraud the church. The same worries surrounded other taxes raised on the fields, such as the *champart* (a seigneurial due). The state would likewise secure its own taxation, which depended on the farmers' revenues, if it reduced the chance of theft.[38]

[35] Only a few agricultural reformers mentioned it in passing at the end of the eighteenth century. See, for example, Archives Nationales (hereafter, A.N.), F10 284.

[36] Articles concerning the posts of *gardes messiers* appear in the *coutumes* of Mons, Artois, Ponthieu, La Rochelle, Amiens, Nevers, Auvergne, Normandy, and so on. Ordinances dealing with their duties appeared in 1402, 1515, 1544, as part of the Forest regulations of 1669, and in the edicts of 1706 and 1709. Guyot, *Répertoire*, 11: 491–92, "messier."

[37] In France, Article 49 of the ordinance of Blois of 1576 established that the tithe had to be collected on the fields. Guyot, *Répertoire*, 6: 15.

[38] On dangers to the standing crop, see, for example, Daniel Hickey, "Crop Stealing and the Village Community in the Seventeenth and Eighteenth Centuries," paper presented at the meeting

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The connection between gleaning and tithing was made explicit in another, contemporary piece of legislation: the *placard* issued in the Low Countries by Philip II.[39] The tithe had to be left on the fields. The crops could not be removed until the tithe had been duly collected. To prevent the theft of such grain, the poor gleaners (*les pauvres gens glennans ès champs*) would not be allowed on the fields until the tither had come by.

The sixteenth century, then, viewed gleaning as a threat to taxation and as a potential breach of the peace. But although the laws protected farmers from damage and theft, they also dispossessed farmers of gleaning. The state was prepared to do this because, as the preamble to the edict of 1554 stated, it believed that gleaning was an activity "that yielded no profit," one that farmers and landowners generously abandoned to the needy. Gleaning was perceived as a marginal activity that could serve only a small group of deserving poor, newly defined as the old, infirm, and widowed. This view of gleaning protected some of the state's interests at the expense of others. By limiting theft in the fields, the state safeguarded important sources of taxation. But, at the same time, it reduced the incomes of the group that provided most taxes, the small farmers. Only those who fell below the threshold of taxation altogether were now permitted to glean.[40]

IN EIGHTEENTH-CENTURY FRANCE, royal legislation on gleaning underwent another transformation. The authorities intervened repeatedly, insisting on their vision of gleaning, regardless of local practice. For Enlightenment authorities, threats to the harvest no longer came from outcasts and miscreants, be they gangs of thugs or local misfits, who abused the privilege of gleaning and imperiled taxation. Time had shown that farmers and laborers interfered as much with the crown's conception of gleaning as did vagrants and other "parasites." If anything, the real threat lay within the villages themselves and the local arrangements they made about gleaning. Gleaning, the crown now believed, was something strictly reserved to the destitute and infirm.

The eighteenth century thus expanded the Renaissance view that gleaning had little economic significance for the farmer and that it should be reserved for the indigent. The statutes therefore placed much more emphasis on who was entitled to glean and especially who was not. The protection of the tithes and other agricultural payments became secondary. The harvest was now overseen in the

of the Society for French Historical Studies, Columbia, South Carolina, March 1988; as well as the prosecutions in A.N. AD III.

[39] The *placard* or law of June 5, 1557, is given in Guyot, *Répertoire*, 6: 31–32. See also the edict of 1576, in Jean Petremand, *Recueil des ordonnances et edictz de la Franche-Comté de Bourgogne* (Dole, 1619) [A. D. Doubs I–746].

[40] A number of recent works have suggested that the French state tried to protect the incomes of small farmers in order to preserve its tax base. See, for example, Hilton L. Root, *Peasants and King in Burgundy: Agrarian Foundations of French Absolutism* (Berkeley, Calif., 1987), which argues that the crown protected the villages' common lands, against usurpations by seigneurs and big farmers, so that villagers might lease them and thus raise money for taxes and communal expenditures. Meddling with gleaning practices did not produce the same results. It lowered the income of villagers by limiting their surplus intake. It also diminished the big farmers' profits by denying them access to the leftovers.

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interests of a carefully delineated group of paupers. Couched in the language of paternalism and philanthropy, the regulations implied a calculated shifting of social obligations. Responsibility for the poor would lie with the farmers.

Thus, in spite of the strong religious overtones of the new legislation, the church was divested of its responsibility toward the poor. The tithes no longer served this purpose, although that is what the early church had intended.[41] Charity became invested in the farmer instead. This situation was noted in the *cahier* of Castenay in Normandy, in March 1789. "And since everyone knows that real wealth comes from agriculture, it is always the farmer who is expected to help the poor. This was the excuse for ordering him to pay tithes; that is why the poor followers of St. Francis turn to him, and why he becomes prey to those bands of vagabonds who periodically ravage the countryside."[42] In France, there were no parish poor rates as in England, but a share of local fines went to the poor, and the vestry maintained a communal poor box. Above all, farmers contributed part of their produce, in the form of gleaning.

The French crown continued to regulate gleaning, mainly through the Parlement of Paris and the provincial intendants, who regularly issued ordinances.[43] The Parlement of Toulouse in 1714, 1722, and 1779, of Flanders and Hainaut in 1731, 1740, 1779, and 1786, of Normandy in 1741 and 1749 (to mention only a few) made similar provisions, and this does not include a myriad of local regulations.[44] Even in the eighteenth century, traditional concerns about labor were evident. Except for the few debilitated or deserving individuals allowed to glean, all others were treated as lazy and made to work at the harvest. This was the quid pro quo offered the farmers: the state would secure them a cheap labor force in return for their abandoning the leftovers.

What began in the sixteenth century as a desire to safeguard the crops and payments in kind solidified into a vision of rural life. For centuries, gleaning had been detached from the harvest proper and reserved, at least in theory, for a minority of destitute villagers. Farmers who gathered the remains themselves were thus robbing the poor. The state had to watch out for such predators. Eighteenth-century judges were truly appalled by what they called the rapaciousness of farmers who continued to glean rather than leave the deserving poor their share. "The gleaning of the poor is a sacred institution, upheld by all the laws of the church, authorized by every state; yet, by the fatality that afflicts all human

[41] See the discussion in Giles Constable, *Monastic Tithes: From Their Origins to the Twelfth Century* (Cambridge, 1964), chap. 1. The population resented this, however. See the complaints in the *cahiers*. The village of Dimont, in the bailliage of Avesnes, reproached the church for not helping the poor during the previous, very harsh winter. "On n'a pu arracher le moindre secours des moines, l'exemple même des Seigneurs et des riches n'a pu les exciter à la commisération"; A.D. Nord, 92J34.

[42] A.D. Seine-Maritime, 4 BP 6010. See also, among numerous examples, the charge that rich farmers who gleaned ought to know better and show some concern for the poor. A.D. Nord, C14776, Avesnes, September 4, 1789.

[43] See the gleaning regulations of July 3, 1778, and of June 10, 1780, and February 16, 1784, A.N. AD IV/1.

[44] For the Parlement of Toulouse, A.D. Haute-Garonne, B1335, July 10, 1714, B1381, August 28, 1722, and B1785, June 1779; by the intendant of Flanders, A.D. Nord C20279, August 10, 1740, and the Parlement of Flanders, Placards 8264, August 1786, the intendant of Hainaut, C10012, June 1731, and C7781, June 1779; the Parlement of Normandy, A.D. Seine-Maritime, 1B1401, July 20, 1741, and 1B1417, July 21, 1749; ordinance of the bailliage of Fresnay-L'Evêque of July 23, 1762, A.D. Eure-et-Loir, B1335.

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institutions, the best of laws always give rise to the most revolting abuses."[45] The poor were entitled to glean by divine law. This right had accrued to them through the ages and had its origins in the Scriptures.[46]

To enforce this vision of communal responsibility toward the poor, the state resorted to an ancient model. Unlike previous harvest regulations, it tied gleaning to Mosaic law and made it the foundation for this form of charity. Indeed, the Old Testament and the story of Ruth offered powerful guides. Gleaning in biblical times had been reserved for the poor, and the tithes had gone to the priests. More important, the landowners had been responsible for the offerings to the poor.

The association of gleaning with the poor was not new, of course. Its biblical origins were plain but were rarely discussed until the eighteenth century and never in the royal or local regulations examined above. French eighteenth-century statutes made such theological justifications explicit.[47] Enlightenment judges and administrators repeatedly based their rulings on the Bible. Contemporary dictionaries used the same authority. Gleaning, an eighteenth-century compendium tells us, "means to pick up the stalks that remain on the field after the farmer has removed his crop. This portion of the earth's bounty belongs to the poor and is allotted to them in a special manner. See Leviticus, Chapter 19, verses 9 and 6, as well as [local customs]."[48] The relevant passage of the Bible states: "And when ye reap the harvest of your land, thou shalt not wholly reap the corners of thy field, neither shalt thou gather the gleaning of thy harvest."[49] For eighteenth-century jurists, the origin of the practice of reserving gleanings for the needy thus lay in the Old Testament, which instructed the Israelites to leave a number of offerings for the poor at the time of the harvest.[50] The Bible also offered an illustration of the custom in the story of Ruth.[51] Ruth had gone to the fields to gather grain for herself and her mother-in-law, Naomi. Years before, during a period of famine, Naomi, her husband, and their two sons had left Bethlehem for the country of Moab. There the sons married local Moabite women. When the men died, Naomi and one daughter-in-law, Ruth, returned to

[45] A.N. AD XVIIIc/104, p. 135.

[46] "La faculté de glaner est un droit que Dieu même a accordé aux pauvres que leur âge ou leurs infirmités mettent hors d'état de travailler." A.N. AD IV/1, "Arrest de la cour de Parlement qui homologue une Sentence rendue en la Justice de Laval, concernant le glanage," February 16, 1784. See A.D. Nord, Placards 8264, "Arrêts du Parlement de Flandre," no. 42, August 2, 1786: "un usage, dont l'antique origine se trouve consignée dans les Livres Saints, et qui nous a été transmise par une longue suite de siècles qui en ont confirmé la possession."

[47] There were similar linkages in eighteenth-century England; see Thompson, "Custom, Law and Common Right," 140; and King, "Gleaners, Farmers," 142.

[48] J. B. Denisart, *Collection des décisions nouvelles et de notions relatives à la jurisprudence actuelle*, 4 vols. (Paris, 1775), 2: 409–10. A similar definition appears in Guyot, *Répertoire*, 8: 159. The same reference to Leviticus appears in David Houard, *Dictionnaire de droit normand*, 4 vols. (Rouen, 1780–82), 2: 625; and in the "Tableau de la législation antérieure au Projet de Code rural," A.N. F10 204 (1806–15).

[49] King James version.

[50] There were a few references to Deuteronomy by French Revolution–era legislators.

[51] The Book of Ruth was added to the Book of Judges sometime between 450 and 250 B.C., although some scholars believe it could have been written as early as 1000 to 700 B.C., bringing it closer to the compilation of the Laws. (Leviticus, around 950 B.C.; Jacob Neusner, *Self-Fulfilling Prophecy: Exile and Return in the History of Judaism* [Boston, 1987], 24–27.) There is a vast literature on the subject, but see, for example, Robert L. Hubbard, Jr., *The Book of Ruth* (Grand Rapids, Mich., 1988).

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Bethlehem, in hopes of reclaiming an inheritance. The two women were poor, defenseless, and in need of a protector. They found one in Boaz, a wealthy relative who took Ruth under his wing, redeemed the inheritance, and married her.[52] The initial meeting between Ruth and Boaz took place in his fields, where she had come to glean. Boaz arrived, noticed the stranger, and inquired after her. The overseer explained that she was the newly arrived Moabite woman and that she had asked for permission to glean after the reapers, a request that he had granted. Boaz extended this favor by allowing Ruth to glean among the sheaves and even ordered his men to drop handfuls for her to gather.[53]

The Book of Ruth thus fixed the image of the gleaner as a poor woman more effectively than any other scriptural writing; Leviticus had presented gleaning as unrelated to gender, but the story of Ruth gave it a distinctly feminine cast. In the process, it also described a properly regulated harvest, in which each performed his or her appointed task and the poor politely requested permission to glean. Jurists used Ruth to illustrate the practice. By the seventeenth century, Ruth and Boaz had become a fairly common iconographic motif.[54] We need only think of Nicolas Poussin's *Summer* scene, now at the Louvre.[55] The summer months were traditionally illustrated with harvest scenes,[56] and Poussin focused his painting of the season on Boaz gesturing to the kneeling Ruth to glean in his fields. In 1690, the *Dictionnaire de Furetière* explained that gleaning consisted of picking up leftover grain in the way Ruth had done in the fields of Boaz.[57] The story of Ruth also inspired poems and a popular opera. The latter prompted François de Neufchâteau, one of the leading figures (and subsequent head) of the commission that devised the Rural Code in the late 1790s, to complain that he did not borrow his economics from the stage.[58]

The Book of Ruth offered a most attractive parallel. Whereas Leviticus referred to the poor in general, the Book of Ruth embodied far better the narrower injunction of Deuteronomy 24: 19–22, which specified that forgotten sheaves, leftover olives and grapes, as well as defective clusters, were gleanings reserved for "the stranger, the fatherless, and the widow."[59] Because eighteenth-century legists meant to restrict gleaning to the widow, the orphan, and the infirm, the image of Ruth served them better. One may wonder at the sloppiness of their biblical references but, in Catholic France, it seems unlikely that many people would point out that Deuteronomy did the job more effectively than Leviticus.[60]

[52] There was some speculation on whether he was required by Mosaic law to marry her, as the closest, and willing, relative of her husband.

[53] Ruth 2: 6–8, and 1: 15–17. Jack M. Sasson, *Ruth: A New Translation with a Philological Commentary and a Formalist-Folklorist Interpretation* (Baltimore, Md., 1979), 38, 48, 54.

[54] The story of Ruth also contained the sort of salacious elements that contemporaries liked to associate with the harvest and the summer heat that awakened desire. Ruth was not only the possible recipient of unwanted attentions from the reapers; she also "seduced" Boaz on the threshing floor.

[55] Nicolas Poussin, "L'été ou Ruth et Booz," 1660–1664, Louvre, Paris.

[56] Perrine Mane, "Comparaison des thèmes iconographiques des calendriers monumentaux et enluminés en France, aux XII^{e} et XIII^{e} siècles," *Cahiers de civilisation médiévale*, 29 (1986): 257–64.

[57] *Dictionnaire de Furetière* (Paris, 1690), vol. 2, s.v. "Glaner."

[58] A.N. AD XXIIIC/310, letter of August 9, 1793. "Je respecte l'histoire de Booz et de Ruth, mais ce n'est pas à l'opéra qu'il faut chercher des notions de morale ou d'agriculture."

[59] Deuteronomy 24:19, King James version.

[60] As the law of the Israelites, the Pentateuch could extend this charity to the wandering members

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Nicolas Poussin, "L'été ou Ruth et Booz" [Summer, or Ruth and Boaz], 1660–1664. The Louvre, © Reunion des Musées Nationaux, Paris.

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This should not diminish the significance of these references. When eighteenth-century Europeans sought a rationale for the custom of gleaning, they found it in the Old Testament. Other sources did not work as well, for they did not embody the sort of high-minded justification that would permit a formal intervention in the harvest. Customary law, for instance, acknowledged the existence of gleaning and specified its timing but offered almost no regulation regarding who had the right to participate. The *Coutume de Péronne*, for instance, spoke only of "le peuple" as having the right to glean.[61]

It was in this period, then, that the biblical examples and the Book of Ruth began to enjoy an appreciable vogue. If the poor had complaints, they should direct them against the farmers who gathered the leftovers. During the eighteenth century, the argument that the poor had the exclusive right to glean became the norm, a group of poor whom contemporary officials defined as the weak and impotent, rather than the settled laboring classes.

FOR EIGHTEENTH-CENTURY OFFICIALS, gleaning thus moved from being a local concern, closely linked to the harvest, to a governmental concern, tied to charity and taxation. The village remained responsible for the procedure but within strictly defined boundaries. As the state shifted responsibility for the poor to the rural community, the courts' definition of abuses expanded. Because there was now only one official version of gleaning—that set aside for the deserving poor—all other variants of the practice were prosecuted. Law courts throughout France forbade farmers and landowners alike to sell the right to glean in their fields, to glean for themselves, to give preference to the wives and children of their harvesters, or to use violence against persons now entitled to glean.[62]

Sometimes, as in this example from Toulouse, judges focused on the laboring villagers rather than landowners. "It has come to the attention of the court that . . . the poor . . . enter the fields and orchards . . . under the guise of gleaning and ravage the crops."[63] In Languedoc, as in other southern provinces, gleaning was permitted as long as the owner consented. Nonetheless, there were complications. "Although the court meant to grant this permission . . . only to those who are truly poor, yet they are the ones who least profit from it because all sorts of people insist on gleaning, although they do not need it, or else they get other people to glean for them . . . while they might usefully be employed at the harvest . . . Hundreds of gleaners of both sexes have banded together and . . . gone into the fields . . . and no one is able to stop them."[64] In the north as well, this abuse

of the tribe, in ways that eighteenth-century society, which feared vagrancy, would not. Both Leviticus and Deuteronomy were cited in England during the same period. See King, "Gleaners, Farmers," 141–42. King, however, does not see how the use of the Scriptures narrows the group of gleaners.

[61] Degrully, *Le droit de glanage*, 51.

[62] Guyot, *Répertoire*, 8: 160, July 11, 1782.

[63] A.D. Haute-Garonne, B1314, June 28, 1710.

[64] A.D. Haute-Garonne, B1413, August 4, 1727. Similar complaints appear in the registers of the Parlement of Paris, for example in 1778, A.N. AD IV/1, "Extrait des Registres du Parlement du 3 juillet 1778."

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of gleaning rights was a major concern. "A great number of peasants [*ménagers*] and able-bodied laborers go gleaning with their wives and children, thereby causing grave prejudice to the poor, to whom gleaning rightfully belongs."[65] These are only two of many examples that show that gleaning was not especially a female activity.

Whatever the nature of the trespass, the contention was the same: the poor were being victimized, and the courts intervened in order to uphold their rights. Here (as in other cases), officials spelled out their understanding of the "poor" who deserved communal support: not laborers supplementing their income but the indigent who had no other means of support. Only rarely did they mention broader economic interests, as when the Parlement of Toulouse in 1722 condemned the theft of crops as harmful to the public at large and to the church in particular, for it lost a great deal of revenue.[66] Economic considerations might figure in the attention to the costs of labor, and the complaints against the "laziness" of peasants who would rather glean—and insisted on their right to do so—than work as laborers in the fields. But the primary argument remained the protection of the poor.

This governmental policy was both unfeasible and unenforceable because it did not correspond to any real situation, at least as most villagers understood it. It interfered directly with the organization of the harvest. It not only detached a crucial segment, the collection of the fallen or leftover grain, it also fundamentally denied its economic importance. The sort of gleaning that the state allowed could benefit only the very poor, who had a limited time to gather (one to three days, depending on the area).

Especially in the cereal plains, the fallen grain was too important to be disregarded in this way. The farmers wanted their share, and if they could not obtain it directly, they meant to offer it as part of their harvesters' wages. In fact, the series of regulations and endless lawsuits discussed below surrounding gleaning show the extent to which the state's vision of the harvest was impracticable.

The gleaning cases fall into a number of categories: struggles among villagers over the right to glean, similar struggles between villagers and landowners, and jurisdictional conflicts between villages. Time and again, farmers were fined for allowing gleaners to follow the reapers.[67] What is more, they continued to hire gleaners. Payments varied. In some cases, gleaners took half of what they collected and gave the other half to the farmer.[68] The more usual procedure was to allow the families of the harvesters to glean. This was doubly economical. It provided labor for all phases of the harvest—the more difficult ones of reaping and binding and the easier task of gleaning. This last step, then, counted as part of the family's wages.[69] As a petition to the intendant explained:

[65] A.D. Nord, C7781, June 6, 1779, gleaning regulations by the intendant.

[66] A.D. Haute-Garonne, B1381, August 28, 1722.

[67] A.D. Nord, C9930, October 11, 1740, C16411, October 1740, C7345, August 1757, C6723, August 13, 1766, C6693, August 18, 1775, C7743, July 31, 1778, C7653, August 14, 1783, C14776, September 1789. See the regulations forbidding this in the village of Castelmaurou; A.D. Haute-Garonne, 2E1035, October 31, 1751.

[68] A.D. Nord, C12540, petition to the intendant by the poor of Villers-Pol, June 19, 1724.

[69] The examples we have suggest that gleanings might account for one or two months of a laboring family's subsistence. In England, gleaning supplied perhaps one-tenth of the family's income. King,

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Over the last few years, [the day laborers of the village of Longueville] agreed to receive from the tenant-farmers one *vasseau* of grain for each *journel* of land [they harvested], which is a very low wage, but with the understanding that the families of these workers would be allowed to harvest and glean. They learned today that this was contrary to the intendant's regulations . . . and so they begged him to allow the gleaners to follow them in the usual manner [*comme Lon at toujours fait*], as they had always done.[70]

Gleaning by harvester families for wages in kind appears to have been a common arrangement, perhaps the normal one in large areas of France. The right to glean formed part of the laborer's contract. Certainly, the authorities believed that the practice was widespread, and they went to great lengths to forbid it. Again and again, intendants and law courts prohibited "landowners and farmers [from hiring] mowers and harvesters with the understanding that their wives and children would be entitled to glean behind the mowers, and [they nullified] any such stipulations, yet without freeing the workers from their other commitments . . . under penalty of 30 livres on behalf of the parish poor." "The wives, children, and laborers of landowners, farmers, and harvesters are likewise forbidden from gleaning among the crops." These injunctions were meant, of course, to preserve gleaning for the poor, "to whom it rightfully belonged."[71]

There was evidently trouble for farmers who prevented gleaning by workers' families. On August 14, 1770, a farmer appeared before the local authorities, covered in blood. Two of his male binders had jumped on him for chasing their sister out of his fields when she gleaned before the appointed time.[72]

In Provence, as in England, women were regularly employed as gleaners, although this was illegal. They received two-thirds of the grain, and the remainder went to the owner. Such sharing was considered more efficient than a daily

"Gleaners, Farmers," 116, and "Customary Rights," 462–63. Elsewhere, we can presume that it provided part of the household's winter supply. When a village in northern France burnt down in 1788, gleaning was so important a resource that people listed it among their major losses. A.N. H1 1469, report on the fire in Caudry, Hainaut, September 6, 1788. One master weaver listed gleaned wheat among losses that included his house, hay for his cow, the tools of his trade, and linen merchandise, all worth 4,000 livres. Another claimed the loss of four persons' gleanings, his house, tiles, wood, and charcoal, all worth 500 livres. Yet another included the gleaning of four individuals, a small barn, and the wheat from one-third of a hectare, for a total loss of 2,000 livres.

Administrative records, judicial records, and notarial records occasionally mention other types of agricultural wages but reveal almost nothing about the quantities gleaned. In one court case, the authorities examined the bundles of gleanings collected by one woman, in order to separate them from stolen sheaves. There were twenty-eight such bundles, each containing between five and nineteen "handfuls." A.D. Eure-et-Loir, B1937, bailliage of Saint-Père-en-Vallée, August 21, 1775. The amount amassed depended, in fact, on the way the activity was conceived: as wage labor or as scavenging.

[70] A.D. Nord, C18328, July 15, 1731; the petition to the intendant was accompanied by affidavits from the local farmers that the local custom was to let the laborers' families glean behind. Similar arrangements existed in other parts of this northern province. C16393, Le Quesnoy, October 1740, where a farmer was accused of allowing his harvesters to glean. He denied it, however. See also A.D. Nord, C6666, Le Quesnoy, July 30, 1759.

[71] A.D. Nord, C10012, June 12, 1731, C20279, August 10, 1740, C7781, June 6, 1779, by the intendant of Flanders. The use of the scythe was widespread, and the crops were mowed. The Parlement of Toulouse made similar prohibitions. A.D. Haute-Garonne, B1335, July 10, 1714, B1413, August 4, 1727, B1636, June 20, 1759, B1786, July 15, 1779. The same "abuses" existed in England; see Thompson, "Custom, Law, and Common Right," 144.

[72] A.D. Nord, C6764, village of Famars.

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wage because it encouraged the gleaners to sift through the fallen stalks carefully. Sometimes, these women used rakes. Then, once they had gone over the ground, the owners or farmers allowed anyone to pick whatever remained.[73] In Normandy, farmers also hired women to glean, despite the laws against this. In one instance, two women gleaned about a dozen bushels of wheat in eleven days, leaving nothing for the village poor, "who were meant to receive all the fallen grain."[74]

In heavily regulated areas, like the north of France, farmers either defied the authorities directly[75] or used subterfuge. There, with the widespread use of the scythe, which shed more grain, the issue of gleaning was of vital importance.[76] With the connivance of the village council and priest, landowners handed out "gleaning permits" to the able-bodied and even well-to-do. Owners explained that, had they not done so, they would simply have lost too much grain.[77] The gleaning laws disrupted agriculture in other ways. During a wretched harvest in 1740, so many weeds had grown around the plants that farmers had no choice but to separate them with their hands. This led to charges that they had "appeared to be gleaning," although the intendant eventually dismissed the case.[78] In another village, a farmer's wife was denounced for gleaning behind the reapers on one of her own fields. She responded that she had only been showing her binders how to bind and that she had been spitefully accused by a woman with whom she had had a fight.[79]

If using one's hands to pick the wheat looked suspicious, raking the fields after the harvest was considered a special affront, since it left almost nothing for the paupers.[80] Jean Hussier, a tenant-farmer from Avesnelles, explained that "he had raked his wheat only in places where the crop had already been stacked, as was the custom in Avesnelles and surrounding villages, . . . to prevent the loss of his produce, and that, moreover, [he] had taken nothing from the gleaners . . . They could still pick up the broken stalks and heads of grain. Besides, it was only fair that the farmer enjoy the fruits of his labor and of his property."[81] He was nonetheless fined 30 livres (reduced from the enormous sum of 1,000 livres to which he had originally been sentenced) for contravening the gleaning laws. Half the money went to the local gleaning warden (*garde glaneur*) who denounced him and half to the parish poor, or to the nearest urban poorhouse. It was not

[73] A.N. F10 284, petitions to the Committee on Agriculture, letter from Tarascon, June 10, 1791.

[74] A.D. Seine-Maritime, 16J59 fonds Godart de Belbeuf, petition of the curé of Ticheville, July 22, 1787.

[75] In some cases, by gleaning on their own fields. A number of prosperous farmers were accused of this. A.D. Nord C5133, Saint-Hilaire, August 23, 1774, and C8260, Floyon, August 18, 1769. In Avesnes as well, there were complaints that rich farmers were gleaning. A.D. Nord, C14776, September 4, 1789.

[76] An extraordinary number of gleaning cases for the northern provinces survive in departmental archives.

[77] A.D. Nord, C7655, village of Limont Fontaine, August 18, 1783.

[78] A.D. Nord, C16399, Ruesnes, August 17, 1740. The subdelegate confirmed that other farmers had faced the same difficulties and that the *maréchaussée* (rural constabulary) should withdraw its case.

[79] A.D. Nord, C7653, Ferrière la Grande, August 22, 1783. "Mais montrait seulement à des ouvriers recueilleurs la façon de recueillir."

[80] A.D. Nord, C7721, letter from the subdelegate of Maubeuge, Baret, to the intendant, August 1, 1778.

[81] A.D. Nord, C14695, October 16, 1789.

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surprising that the authorities were eager to prosecute crimes and misdemeanors that filled the poor box in this way or that the local and regional police forces were diligent in prosecuting offenders. In fact, gleaning wardens demonstrated an exemplary zeal. They drew up report after report—and the archives still bulge with their little slips—on each of their rounds, involving the same culprits, who were eventually fined.

In response, farmers and other villagers vigorously defended property owners' freedoms. Arguments that farmers should be free to dispose of their own property became more insistent in prerevolutionary France, not only among Physiocrat intellectuals but among villagers themselves. This was no more than would be expected in a period when markets were expanding and farmers were eager to exploit all of their produce, the same produce that the state, rather than the community, meant to deny them. When in 1769 a villager was found gleaning among his crops with his aunt, he explained that "he thought that since he had bought and paid for his property, he could do with it as he pleased [*il en etoit le maitre*]."[82] The poor, non-professional gleaner increased the farmer's sense of invasion. In Languedoc, a townswoman found raking on a suburban property was injured by the owner. Similar incidents were reported elsewhere. A farmer (*laboureur*) near Chartres hit a seventeen-year-old girl, shouting, "I won't let you cut the hay on my field, you slut, get off my property." The son of the mayor of Marienbourg chased gleaners off city property and broke one woman's arm.[83]

Small holders shared this sense of invasion with large tenant-farmers, and they were warned to watch out for gleaners. "The Widow Portehauts told the witness's wife that he ought to be guarding his field rather than hanging around the house, for she had seen six women gleaning among his crops."[84] One estate overseer was called at 3 a.m. because people had seen two men and three women, described as harvesters (*solatiers du sol de la dîme*) gleaning on her master's fields. She tried to stop them. They refused to leave, answering that they would do as they pleased and glean "the heads of grain left for the poor." When she insisted, they beat her up.[85]

Farmers and landowners were not the only ones upset by the legislation. The restrictions on gleaning were also disruptive for the laborers. As we have seen,

[82] A.D. Nord, C8264, Sémeries, August 17, 1769. In fact, this argument dominated the discussions of the Rural Code.

[83] A.D. Haute-Garonne, 5G54, Officialité de Toulouse, temporalité, Saint-Martin de Labordes, July 22, 1774; A.D. Eure-et-Loir, B199, bailliage de Gallardon, August 1751; A.D. Nord, C10931, petition to intendant, September 1730.

[84] A.D. Eure-et-Loir, B909, bailliage de Janville, September 1741.

[85] A.D. Haute-Garonne, 5G54, Officialité of Toulouse, temporalité, Balma, July 20, 1773. Gleaners were not the only thieves. The danger of theft was widespread and not limited to the tithes. This was especially true in areas of small and middling property or of absentee landowners, such as Languedoc. See A.D. Haute-Garonne, 5G54, officialité de Toulouse, temporalité, Roussiac, August 1770, for complaints about the theft of grain in the countryside, especially at night. See also 101B378, Capitoulat of Toulouse, July 1789, the complaint of a *bourgeois* of Pibrac. He had gone out to look at the crops on his fields, seen women gathering hay (*de l'herbe*), and he judged that it was impossible for the one using a scythe not to be cutting his crops at the same time. In order to prevent such thefts, peasants often spent the night in the fields, watching over their crops. A.D. Haute-Garonne, B juridictions secondaires, Muret 428, May 24, 1757, and A.D. Eure-et-Loir, B1630, Saint-Aubin, September 1788.

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they had long associated gleaning rights with participation in the harvest and resented a transferral of rights to those who had not worked to bring in the crops.

> However, Your Excellency, the workers in the coal mines, who temporarily occupy a few houses in the village of Saint-Vaast, have claimed since last year the right to glean [on this territory] and to exclude the inhabitants of Aubry, who suffer gravely from this, as do the tenant-farmers. This claim is especially unjust and ill-founded, because they would be alone in gathering the fruit of other people's labor.[86]

At worst, the legislation might actually create the "leisure preference," of which many officials complained.[87] If gleaning was to be a separate activity, reserved for the "poor," rather than the harvesters, then the villagers were going to make sure that they still profited from it. They would give up harvesting, stand guard by the fields, and rush to pick up the leftovers the moment the signal was given. "[The lack of hands can be attributed to the fact] that able-bodied men and women shun the wages that they might earn during harvest and instead take part in the cowardly occupation of gleaning, for what attracts them is [the] ability to grab the wheat stalks with impunity and at their discretion."[88]

More often, however, they simply tried to restore the *status quo ante* by harvesting the old way. Gleaners would enter the fields along with the other workers. "At 5 a.m. on August 6, Rousseau's wife went to the Formarville farm, where the wheat was being harvested, in order to glean with the other women and inhabitants of the parish of Bazoches."[89] Peasants continued to glean at irregular times, that is, before the end of the harvest.[90] In one village, the inhabitants began to glean while the carts were still on the fields, before the crop had been completely bound into sheaves. "One rebellious individual [*mutin*] started it all, and, since he refused to budge, all the other gleaners followed his example."[91] Near Provins, "a prodigious number of male and female gleaners of all ages and occupations, be they single or married, men or women, throw themselves into the fields the moment the harvesters enter them. They will not let what escapes them go to the poor as they are supposed to."[92] Not far from there, three female harvesters, charged with illegal gleaning by the local guard, explained that they had "taken advantage of the lunch break to glean in those areas where it is permitted, in order to alleviate their terrible poverty and provide a little food for their families."[93] The gleaning laws stimulated conflicts in the countryside but not

[86] A.D. Nord, 11G101, Saint-Vaast, petition of the inhabitants of Aubry to the intendant of Hainaut, August 21, 1763. Since provincial law limited gleaning to parish boundaries, their request was refused.

[87] This argument appears in most official documents on gleaning as a preamble to legislation or as an argument in favor of such legislation.

[88] A.N. G7 727, *Mémoire*, n.d. There follow the obligatory remarks about the deserving poor, the aged, and the orphans who need such resources as much as the farmers need their labor force.

[89] A.D. Eure-et-Loir, B968, bailliage of Janville, August 1781. This led to a territorial dispute.

[90] See A.D. Haute-Garonne, B juridictions secondaires, Muret 422, July 1778.

[91] A.D. Nord, C6793, letter to the intendant, August 12, 1772.

[92] A.N. F10 284, petitions to the Committee on Agriculture, parish of Monceaux-les-Bry, district of Provins, June 21, 1791.

[93] A.D. Nord, C6723, Saint-Hilaire, August 11, 1766.

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the ones we might necessarily expect. They pitted village communities against the law and allied farmers and laborers against the very poor.[94]

In some areas, gleaning was restricted to the local residents. This law was intended to reduce vagrancy and make parishes responsible for their own poor. But it provoked a series of territorial conflicts in the countryside, as people tried to stop inhabitants of other parishes from gleaning on their plots, even though the others had done so for centuries.[95] "The poor who used to glean beyond their parish boundaries, without paying heed to any regulation, when faced with these restrictions, grumbled and were so bold as to go together to glean beyond the limits of their territory, saying loud and clear that they did not care about the intendant's orders."[96]

Since the right to glean was often part of the harvesters' wage, despite the legislation, this attempt to confine gleaning to parish residents naturally interfered with patterns of hiring, either from surrounding villages or from other regions.[97] Farmers would not stand for this restriction, however. In 1724, the farmers of a northern village, Villers-Pol, who used non-resident harvesters (also entitled to glean), argued that they "were free to chose whomever they liked."[98] Some were even willing to go a step further and challenge the whole system. Landowners in Hainaut insisted that "gleaning was not a right but simply a charitable act on the part of property owners and farmers."[99]

Thus most villagers, rich and poor alike, opposed the new restrictive legislation. I have argued that this opposition reflected the laws' interference with longstanding agricultural practices, especially those concerning distribution of work. Labor allocation and remuneration were crucial on large farms, especially in market-oriented regions such as the north. There, the state's interference with labor arrangements clearly cut into the farmers' profits. Yet opposition to the gleaning laws was too widespread to reflect a minority of interests, however powerful and vocal. Rural inhabitants throughout France defied and denounced the new laws.

[94] This was not part of a general rebellion against the state and state intervention. Villagers were quite ready to turn to the state for redress of local grievances when they felt this sort of meddling was to their advantage. See Judith Miller, "Politics and Urban Provisioning Crises: Bakers, Police, and Parlements in France, 1750–1793," *Journal of Modern History*, 64 (1992): 227–62; Root, *Peasants and King in Burgundy*; and Liana Vardi, "Peasants and the Law: A Village Appeals to the French Royal Council, 1768–1791," *Social History*, 13 (1988): 295–313.

[95] See, for example, A.D. Eure-et-Loir, B968, bailliage of Janville, August 1781. Also A.D. Nord, C6661, Landrecy, October 1759, C6368, Bavai, July 1772, C8244, Felleries, July 1768, C7646, Bersillies, 1782–83, C9929, Bavai, September 1740, C6668, Jolimetz-Potelle, 1759, C9077, Bavai, August 1766.

[96] A.D. Nord, C6651, copy of letter sent to the Parlement of Flanders, August 25, 1760.

[97] See, for example, A.D. Nord, C17408, Le Quesnoy, 1738. In one case, French farmers had been hiring their harvesters from the Austrian Netherlands, across the border, who then gleaned. The local peasants complained to the intendant, who agreed that this caused "a grave prejudice to the poor subjects of the king." A.D. Nord, C9077, Bavai and Valenciennes, August 1766.

[98] A.D. Nord, C12540, Villers-Pol, June 1724. This petition from the poor peasants to the intendant of Hainaut touches on the issue of the local monopoly of labor. At various times, peasants tried to force farmers to use local laborers; at others, the farmers sought to prevent local workers from going elsewhere. This was especially true in areas where labor was scarce. A.D. Seine-Maritime, 16J21 fonds Godart de Belbeuf, bailliage of Cany, letter of August 12, 1785, concerning the emigration of Norman harvesters to England. A.D. Haute-Garonne, B1358 (edict of 1718), 1G967, Castelmaurou, laborers forbidden to hire themselves out above the set rate or to leave the village, 1719, 1723.

[99] A.D. Nord, C7126, response of the inhabitants of villages surrounding Bavai, 1756.

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In many instances, these were either people insisting on gleaning on their own land or determined that all community members—not just the very poor—share in gleaning.

How are we to understand the state's insistence on regulating such practices? It is striking that officials avoided economic justifications for their regulatory efforts. They did not mention the economic improvements of the eighteenth century or the enhanced profits that farmers enjoyed as a result of free-trade policies. Nor did they speak of laborers' immiseration over the century, either as a special reason for charity or as a source of social dangers.[100] Gleaning was not offered as a supplement for laborers and petty landowners experiencing hardships. Rather, official rhetoric was religious and moral. Administrators retained longstanding concerns about theft, and they grounded gleaning in biblical injunctions to be charitable. They did not, in this context, treat poverty as in any way structural, something that involved a significant proportion of the rural population. The poor whom they envisioned as entitled to glean were a small body of paupers, those truly unable to earn their living in other ways, a group that had always been present in the countryside.

The state, then, did not treat gleaning as a general palliative to poverty. Parlements and intendants cranked out gleaning regulations following the periodic crises that shook the economy, as in 1709 and 1740, but they did so at other times as well, when food was abundant. At the core of regulation, I have tried to show, were both practical concerns and something grander. Regulation of gleaning was partly an effort to control the harvest and in this way to protect taxes and tithes; villagers were to be kept off the fields while these were being collected. At the same time, officials infused gleaning with symbolic significance and turned it into a metaphor for charity, a version of the moral commonwealth. Gleaning served as a symbol for the expectation that poverty could be defined, limited, and contained. In this, the state proved highly successful in turning elite discussions of gleaning away from issues of labor, taxes, and theft and toward those of morality. The rural population, meanwhile, refused to view it in those terms.

Eighteenth-century officials saw their motives as purely charitable and justified their intervention in terms of their wardenship of the poor. They used this rhetoric to legitimize their meddling in the agrarian economy. But they failed to structure and control the gathering of crops. Even after the intensive efforts during the eighteenth century, the harvest did not fall into the sharply distinct phases that the crown envisioned. As the litigation indicates, gleaners continued to be part of the many workers out on the fields.[101] The state proved unable to stop this age-old practice because it was a crucial step in the harvest. The village community continued to regard gleaning as part of the harvest cycle and to claim it as part of their wages. It was the state that insisted on the peasants' moral and

[100] For older and more recent versions of this thesis, see Ernest Labrousse, *La crise de l'économie française à la fin de l'Ancien Régime et au début de la Révolution* (Paris, 1944); and Robert M. Schwartz, *Policing the Poor in Eighteenth-Century France* (Chapel Hill, N.C., 1988).

[101] A.D. Haute-Garonne, 5G54, Officialité de Toulouse, temporalité, Vallègue, August 1773. "Etant occupée à glaner dans un champ . . . dans lequel il y avoit aussi d'autres glaneurs et glaneuses, que le nommé Etienne liait de la gerbe dans le même champ."

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fiscal responsibility toward the needy and the state that defined who the needy were and what they deserved.

THE PICTURE OF POOR FEMALE GLEANERS presented by Millet thus embodied three centuries of official propaganda concerning the poor and the nature of gleaning. Like the state, Millet created an image distinct from the realities of the harvest. In the nineteenth century, farmers and village communities still refused to conform to this representation. The agricultural regulations devised during the French Revolution admitted as much, by allowing local customs and agreements to stand, although the revolutionaries also tried to limit gleaning to only the deserving poor.[102] The nineteenth century could not impose uniform legislation, and local practice continued to vary. In some areas, gleaning was abandoned altogether, in others it was remunerated, and elsewhere it was left to the poor.[103] Millet claimed to have painted from life. He probably did observe women gathering fallen grain in the Parisian countryside. His painting, however, both juxtaposed them with the harvest and removed them from it. In this, it repeated the polarities that the Old Regime had worked so hard to create.

[102] A.N. AD XVIIIC/103 and AD IV/26, *Projet de décret des lois rurales*, 1791; F10 204, 211; and AD IV–26, Projet de Code Rural (1808); AD IV/27, Projet de Code Rural (1858).

[103] See, for example, the *Recueil des usages locaux du département du Nord* (Lille, 1856); Victor Fons, *Constatation et codification des usages locaux agricoles en Haute-Garonne* (Toulouse, 1938); G.-A. Coupelon, *Usages locaux du département de Puy-de-Dôme* (Clermont-Ferrand, 1871); Georges Petit, *Usages locaux suivis comme loi dans le département de l'Yonne* (Auxerre, 1923).

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Voter, Junker, *Landrat*, Priest: The Old Authorities and the New Franchise in Imperial Germany

MARGARET LAVINIA ANDERSON

> Precisely in epochs of revolutionary crisis, people anxiously conjure up the spirits of the past to their service and borrow from them names, battle slogans and costumes in order to present the new scene of world history in this time-honored disguise and this borrowed language . . . The beginner who has learnt a new language always translates it back into his mother tongue.
>
> Karl Marx, *The Eighteenth Brumaire of Louis Bonaparte* (1852).

> Das Junkertum kann sein Testament machen.
>
> *Görlitzer Anzeiger*, April 4, 1871,
> on the consequences of the new election law.

On a cold day in March 1871, the new German empire held its first elections based on direct, equal, manhood suffrage—the most progressive franchise in Europe. Although some of the citizens of the short-lived North German Confederation had made use of such a franchise three years earlier, for many Germans this was their first experience with the democratic suffrage. Far more than Disraeli's Reform Bill of 1867, which left approximately two-thirds of the male population of Britain without the vote, Bismarck's revolutionary new franchise was a "leap in the dark." What would happen was anyone's guess.[1]

Manhood suffrage in Germany was only one of a concatenation of dramatic European events that, in the words of one observer, shook "the bases of human society . . . to their core." Within the previous five years, two great powers had been brought to their knees, the throne of Hanover and the thousand-year-old Papal State had fallen, a revolutionary Paris commune had (however briefly) seized power, and two new nation-states had been born.[2] In Germany, the novelty of the franchise contributed to the heightened sense of revolutionary possibilities, acting as a solvent on inherited political assumptions. Attitudes and actions

My thanks to Professors Thomas A. Brady, Jr., James J. Sheehan, and the *AHR*'s anonymous readers for their helpful comments on this essay.

[1] The two-thirds figure is from D. E. Butler and James Cornford, "United Kingdom," in Stein Rokkan and Jean Meyriat, eds., *International Guide to Electoral Statistics*, vol. 1: *National Elections in Western Europe* (The Hague, 1969), 333.

[2] Stenographische Berichte über die Verhandlungen des . . . Deutschen Reichstages (hereafter, SBDR), April 22, 1871: 328. The *Görlitzer Anzeiger*, Nr. 1, January 1, 1871, led with a survey of the revolutionary events of the preceding year.

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Bavaria: "One more election, and we won't even be able to see the Lord God anymore." (The stickers say "Vote Center.") From *Simplicissimus*, 15, no. 37 (December 11, 1911). Artist: Wilhelm Schultz.

Germans might previously have considered inappropriate (or not considered at all) now appeared imperative. The political culture of Germany may have been more fluid in the late 1860s and early 1870s than at any other time until the upheavals of 1918—perhaps even until 1945.[3]

[3] I have earlier argued for such a fundamental break in continuity for the Catholic population (36 percent), whose voting patterns were realigned in the 1870s in a bloc that held until March 1933.

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Although historians have long noted the anomaly of a democratic franchise in a legendarily authoritarian political system, only recently have they attempted to analyze the meaning of democratic elections for German political culture. A more typical strategy has been to suggest that the intimidation of voters on the one hand and their manipulation on the other undermined whatever challenges manhood suffrage might have posed to existing attitudes and structures of authority.[4] Yet the fact remains that large numbers of Germans took the trouble to go to the polls. Beginning with a respectable turnout of 51 percent in 1871 (a remarkably high figure, considering the number of men still on active military duty because of the French war and thus barred from voting), the German public's commitment to the franchise did not decline but rather grew over time.[5] By the decade before the war, participation averaged over 80 percent, similar to nineteenth-century American turnouts outside the South and, by U.S. standards today, phenomenal.[6]

During the last decade, the drive to examine politics from below has resulted in an avalanche of valuable contributions to party history, as well as case studies of towns, election districts, and regions, often powerfully influenced by modernization theory, communications theory, and quantitative methods.[7] But the more

Margaret Lavinia Anderson, *Windthorst: A Political Biography* (Oxford, 1981), 192–98; and "The Kulturkampf and the Course of German History," *Central European History*, 19 (March 1986): 82–115. Recently, based on a much larger sample and a trajectory extending from the mid-1860s to the 1980s, Karl Rohe has reached the same conclusion. Karl Rohe, *Wahlen und Wählertraditionen in Deutschland* (Frankfurt, 1992).

[4] The emphasis on intimidation appears frequently in contemporary reporting by oppositional parties. "Manipulation" is associated with the "Bielefeld school" of Hans-Ulrich Wehler and colleagues such as Hans-Jürgen Puhle, work that was especially prominent in the 1970s. Although the starkness of their picture has been attacked, the revisionists themselves sometimes resort to similar concepts. David Blackbourn considers "demagogy" a distinct "mode of politics on the German right before 1914," especially in "The Politics of Demagogy," *Populists and Patricians: Essays in Modern German History* (London, 1987), 217–45, esp. 218. One of the first sustained evaluations of the manipulation thesis as applied to voting behavior was Karl Rohe's "Konfession, Klasse und lokale Gesellschaft als Bestimmungsfaktoren des Wahlverhaltens: Überlegungen und Problematisierungen am Beispiel des historischen Ruhrgebiets," in Lothar Albertin and Werner Link, eds., *Politische Parteien auf dem Weg zur parlamentarischen Demokratie in Deutschland: Entwicklungslinien bis zur Gegenwart* (Düsseldorf, 1981), 109–26. The 1978 Habilitationsschrift of the political scientist Peter Steinbach, part of which has appeared as *Die Zähmung des politischen Massenmarktes: Wahlen und Wahlkämpfe im Bismarckreich im Spiegel der Hauptstadt- und Gesinnungsprese*, 3 vols. (Passau, 1990), also subjects the manipulation argument (in spite of a title that evokes it) to criticism based on a combination of communications theory, press analysis, and good sense.

[5] Stanley Suval, *Electoral Politics in Wilhelmine Germany* (Chapel Hill, N.C., 1985), takes high voter turnouts (particularly during the decade before 1914) as his starting point and is informed by broad reading in American electoral history. It is an admirable exception to stereotypic views of German political behavior.

[6] William E. Gienapp, "'Politics Seem to Enter into Everything': Political Culture in the North, 1840–1860," in Stephen E. Maizlish and John J. Kushma, eds., *Essays on American Antebellum Politics, 1840–1860* (Arlington, Tex., 1982), 14–69, is a good introduction to these issues.

[7] To list the literature of the last two decades relevant to electoral politics would overwhelm this article. Longitudinal studies of particular districts have been appearing regularly since the 1950s. A useful early summary was Klaus Müller, "Das Rheinland als Gegenstand der historischen Wahlsoziologie," *Annalen des historischen Vereins für den Niederrhein*, 167 (1965): 124–42. A critical review of recent social-scientific developments is Jost Dülffer, "Historische Wahlforschung," *Neue Politische Literatur*, 2 (1988): 432–40, which concludes that "simple correlations between election results and social or confessional data at high levels of aggregation do not tell us all that much, or what they are made to tell us is, on account of the alleged objectivity of figures, frequently overestimated"; p. 432.

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anthropological aspects of elections, the "inside" as well as the "outside" of the story, including the meanings Germans invested in the act of voting, have not received comparable attention. The following essay will neglect the intricate middle level of analysis that would include discussions of the different parties, issues, regions, and individual election campaigns—on which there is already a vast literature. It will look instead at the quotidian experience of voting itself. Although I shall refer to trajectories of political development over the whole course of the German empire (1871–1914), I shall concentrate on the decades of the 1870s and 1880s. There we shall see how universal suffrage, while not changing the structure of power or composition of elites, set in motion a revolution in social attitudes that shook the deference that had governed German social relations for centuries. Let us begin by looking briefly at imperial Germany's first election, as experienced in three precincts.

THE SUMMONS TO ALL GERMAN MEN TO PARTICIPATE in the new political nation by voting for a Reichstag (parliament) was met with characteristically German attention to duty. In the little town of Liebenswalde, near Potsdam, the commanding officer passed out Conservative ballots to his men and marched them to the polls. In Einbeck, in the former kingdom of Hanover, a constable stationed himself in front of the polling place and confiscated all those ballots that named the legitimist (Guelf) candidate. Shortly thereafter, however, he returned and handed the ballots back, explaining that he had just learned from his superior officer that this was a free election.[8]

In Oberhaid, a Bavarian village near Bamberg, the new legalities also ran up against old certainties. Royal Pastor Keck was disturbed to learn from his parishioners that outsiders—forest wardens, commons keepers, town criers, and postmen—had been coming into the village to campaign on behalf of one Herr Dr. Schmitt, the Liberal candidate. As Pastor Keck told it, he had always been "fundamentally opposed to every kind of election agitation." But, under the circumstances, he considered it "his duty to suggest to his parish, in a suitable fashion," the candidate put up by the Patriots—the Bavarian affiliate of the (Catholic) Center Party. Not wanting to proceed on his own, however, the priest sought to coordinate his efforts with Mayor Fösl and the mayor's deputy, Innkeeper Wagner. But here Keck ran up against unexpected difficulties. The innkeeper, while declaring that "it was all the same to him, et cetera," allowed as how the Liberal candidate was a pretty solid fellow. The mayor temporized. Since he had been summoned to appear at the county seat the next day in any case, to

A more schematic (and sympathetic) survey, which puts various German schools in the context of French electoral history, is Peter Steinbach's "Reichstag Elections in the Kaiserreich: The Prospects for Electoral Research in the Interdisciplinary Context," in Larry Eugene Jones and James N. Retallack, eds., *Elections, Mass Politics, and Social Change in Modern Germany: New Perspectives* (Washington, D.C., 1992), 119–46. Rohe's *Wahlen und Wählertraditionen in Deutschland* with sixty pages of tables and twenty of bibliography, probably represents the current state of the art.

[8] SBDR, March 24, March 28, 1871: 11–12, 25.

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receive instructions about how to conduct the election, he promised to find out then for whom the village was supposed to vote.[9]

Thwarted in his attempt to produce a united front, the priest determined to take matters into his own hands after all. Election day was also the feast of St. Kunigunde, and at eight in the morning, the entire village, including mayor and innkeeper, assembled for Mass. After the sermon and prayer for the king, Pastor Keck reminded his congregation of the importance of this election and, adopting "a conversational tone"—as the pastor himself described it—he bade them assemble afterward in front of the school, where he would suggest to them "the person who had his confidence" (*Vertrauensmann*). He hoped they would give him this sign of their trust, since he could vouch for his having, during his nine years with the community, given them only good advice. The mayor, who had already begun to distribute Liberal ballots, fumed.

The school was also the polling place. As Pastor Keck, followed by his flock, strode over with the Patriot Party's election proclamation in hand, Mayor Fösl hurried up to him and asked him to step inside; he wanted a word with him. When the priest demurred, the mayor warned him that political discussions in the polling place were illegal. That more was at stake in Oberhaid than simply the choice of a Reichstag deputy became clear when Fösl shouted out that Keck "had cast suspicion on him in church" by claiming to have always given good advice. That, protested the mayor, no slouch at decoding a subtext, "sounded as if *I* had been giving bad advice!"[10]

The quarrel quickly became a brawl. The indignant villagers sided with their pastor, forcing the humiliated Fösl, shielded by his daughter (who seems to have had some words of her own with the priest) to retreat into his house. The priest, after remarking on the arrogance of the young lady ("Naseweisheit" was his word) and the low cultural level of her father, distributed two hundred ballots for the Patriot Party. The election result, a 125 to 23 victory for the Patriot candidate, Keck considered a "beautiful" vindication of his conduct. But the mayor, who had given a speech and distributed ballots on behalf of the Liberal candidate, lodged a protest with the Reichstag, charging that clerical influence had tainted the election.

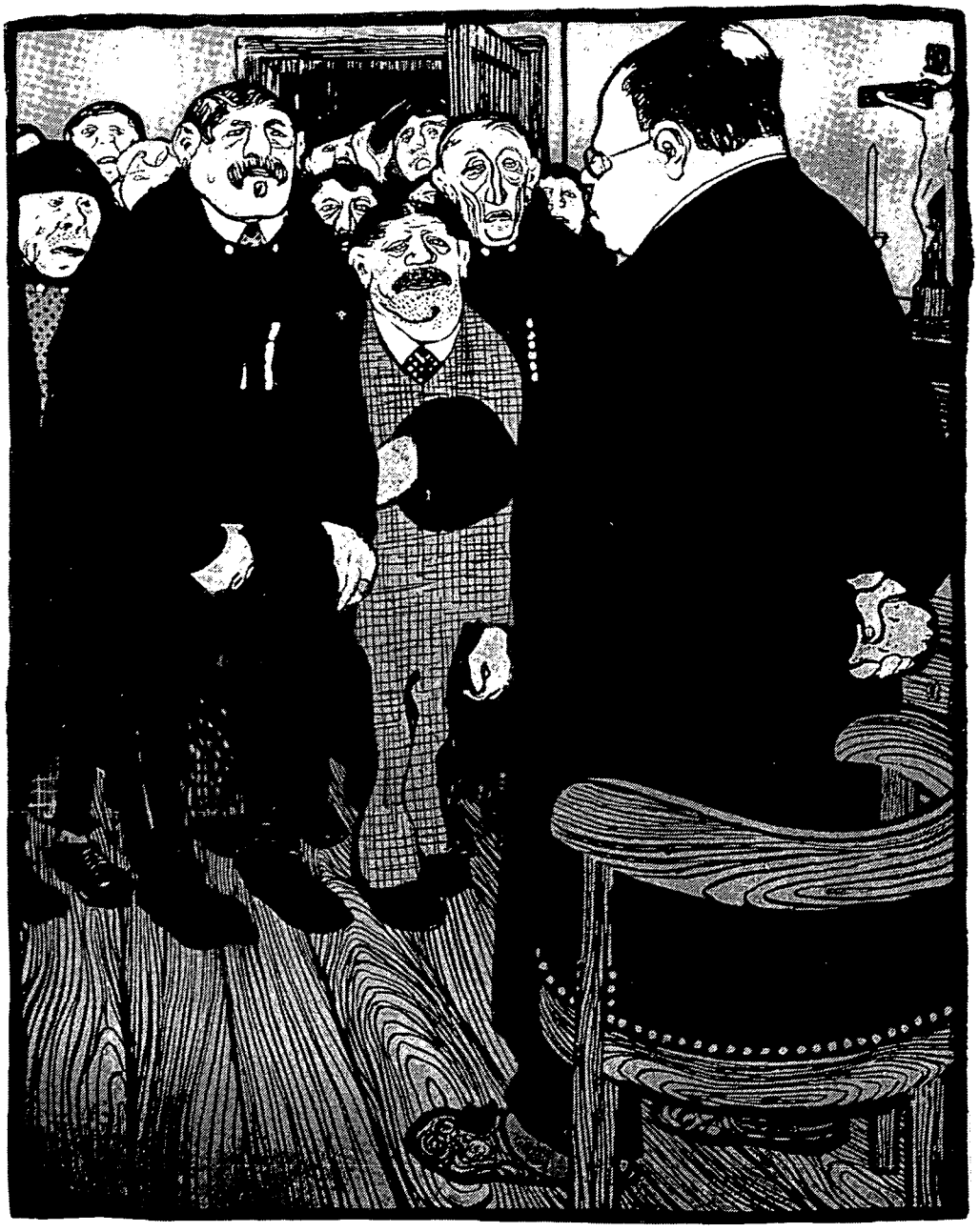
ALTHOUGH THE NEW DEMOCRATIC ELECTION LAW had changed the definition of the political nation and summoned all adult males to the polls, no one accustomed to having a say in Germany—not the garrison commander near Potsdam, not the constable in Hanoverian Einbeck, and certainly not the worthies in Bavarian Oberhaid—seemed to expect that the choice of Reichstag deputies would actually be made by the citizens themselves.

Who *would* make that choice and on what basis? A restricted franchise and open balloting, like that of Britain's, implied that each vote was in some sense a public trust, for which each voter could and should be held accountable by the

[9] Report of Abteilung (hereafter, Abt.) V, April 3, 1871, SBDR, Anlagen I, Aktenstück Nr. 27: 80–83; SBDR, April 17, 1871: 228–43; quote on 238.

[10] I have changed the third person in Fösl's recorded testimony to the first.

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Agitation: "Dear confessants, I am far from wanting to influence your vote. But I have to tell you this: anyone voting Farmer's League loses his eternal salvation!" From *Simplicissimus*, 3, no. 12 (1898–99): 96. Artist: Bruno Paul.

community.[11] Universal suffrage and secret balloting, by contrast, implied at least a potential separation of the individual's interests from those of the state or community and a personal responsibility for political choice. But the contretemps

[11] Norman Gash, *Politics in the Age of Peel: A Study in the Technique of Parliamentary Representation, 1830–1850* (New York, 1953).

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in these three little towns indicates how very foreign such a conception was to those used to having authority. Perhaps Innkeeper Wagner, who had ventured to praise the Liberal contender as a solid fellow, was expressing a personal preference for a candidate or party or issue, but, if so, he was careful to step back from any suggestion that his choice might be political. We cannot assume even a personal preference in the campaigning of his two neighbors. In the end, Mayor Fösl distributed Liberal ballots; but if he was a man of Liberal inclinations, this preference was unknown to the pastor, who had expected agreement on the Patriots' nominee when he first approached him. Mayor Fösl's own remarks suggest that he saw his village as on the receiving end of instructions from above and his own role as a conveyor of the wishes of the Bavarian state (*Obrigkeit*).

And Pastor Keck, interrogated as a result of the mayor's protest, took indignant pride in denying that he had any personal interest in the election or, indeed, in politics whatsoever. In the past, he had been the delegate of several villages to the electoral college that chose the deputies to the Bavarian state diet, and we may take him at his word that he had been elected without any campaigning on his part, in fact, without even voting himself. In Keck's own view, his repeated, unsolicited selection to represent his villagers in these important decisions was itself proof of his personal—and thus political—disinterest.[12]

Whose interest then did Pastor Keck think he represented, if not his own? The term the priest used to characterize the Patriot candidate to his congregation—his "Vertrauensmann"—is revealing. Literally, the word means "the man who has my confidence." But the meaning most immediate to his listeners would have been the figurative one: "my deputy." It is clear that Keck believed in his own, broadly representative function in Oberhaid: a function implicit in his dual appointment by both church and crown (he always insisted on his full title, *Royal* Pastor Keck) but based also on his long association with the community as pastor, confirmed by their repeated selection of him as their delegate to elect the district's deputy to the Bavarian diet, and ratified—so he now argued—by the community having voted overwhelmingly for his candidate over the mayor's. Both Keck's behavior (including his mistaken assumption that he would find agreement from the village's other two leaders) and his subsequent testimony reveal his assurance that his intervention in the Oberhaid election was legitimate precisely because he was not offering his individual, private opinion. That would have been "election agitation"—intolerable and illegitimate.

Here are assumptions that had been widespread in Germany for a long time: the implicit pejorative connotation of "agitation" and its association with factional—that is, selfish—interests and the conviction that such interests were contrary to the obligation of a community leader—pastor, civil servant, man of respect—to

[12] The system of indirect balloting in state elections, by obscuring the connection between voter decision and the ultimate party-political composition of the legislature, no doubt muffled the role of "party" on the village level; Bismarck believed, however, that it enhanced the effectiveness of partisan politicking on the outcome of the final election: Bismarck's Memoranda to the Prussian Staatsministerium, December 23, 1864, and May 23, 1866. Bundesarchiv Potsdam (hereafter cited as BA Potsdam), Reichskanzlei Nr. 685, 11–12v, 13–16v, 17–19. Compare Klaus Erich Pollmann, *Parlamentarismus im Norddeutschen Bund, 1867–1870* (Düsseldorf, 1985), 70.

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represent the *common*weal.[13] Pastor Keck took it for granted that the community was one. He assumed that it expected him to articulate its own interests and convictions. It was not only Germany's civil servants (*Beamtentum*), as Hegel represented them, who laid claims to being the universal class.

Whether the commonweal meant the state, the church, or the village, however, was not a question someone like Royal Pastor Keck could have asked at this point, as these were "wholes" that were themselves not strictly differentiated in the minds of many Germans in 1871. (Keck seems to have been genuinely unaware of his implicitly oppositional stance, although the last two, very heated elections to the Bavarian diet in 1869 had put the Patriot Party in direct conflict with the Bavarian government.) One of the first consequences of the new democratic elections in 1871 was the beginning of the process of differentiation, a process not without explosive possibilities, as the brawl in front of the Oberhaid schoolhouse shows.

But the priest and the mayor, for all their personal rivalry, shared the same view of their political responsibilities, and their view was very different from that implied in the new election law. Although constitutional scholars insisted that voting was not the "right" of an individual but a "responsible public office entrusted to him by the whole,"[14] direct, secret balloting did imply that one's central political decision was indeed a private, individual one. This was a revolutionary assumption. To ground the commonweal on the decisions of individuals, to displace such a highly important public matter as the choice of a national parliament onto the private sphere, implicitly reversed the values traditionally ascribed to public and private activity and, consequently, reversed what was legitimate political influence and what was not. Yet it should not surprise us that the officials and worthies of Oberhaid—like the commander in Liebenswalde and the constable in Einbeck—missed these novel implications and tried instead to fit the new forms into the old categories of their own previous experience. How could it have been otherwise?

Imperial Germany had no Hatch Act restricting the electioneering of government officials. Only Bismarck—briefly—wanted to remove public officials from political life.[15] Instead, time and time again, political spokesmen, both in the government and in the Reichstag, insisted that holders of public office had the same right to agitate for their views as anyone else. But, they were compelled to add, it was their individual, private opinions only that these men were, in this New Dispensation, allowed to express.[16] The election in Oberhaid was thrown out.

[13] Compare James J. Sheehan, *German Liberalism in the Nineteenth Century* (Chicago, 1978), 17, 104–05, 150–51, 235.

[14] Ernst-Rudolf Huber, *Deutsche Verfassungsgeschichte seit 1789* (Stuttgart, 1963–), 3: 864. The public aspect of the franchise was argued by theorists as different as Paul Laband, Georg Jellinek, and Julius Hatschek. See Hatschek, *Kommentar zum Wahlgesetz und zur Wahlordnung im deutschen Kaiserreich* (Berlin, 1920, completed in 1914), 184–85.

[15] The debate over this issue was much more vehement than the debate over the franchise itself, as Georg von Below noted, *Das parlamentarische Wahlrecht in Deutschland* (Berlin, 1909), 2.

[16] See the letter of the Regierungs-President of Lublinitz, von Biebahn, to the County Office (*Landratsamt*), Report of Abt. III, November 8, 1871, SBDR, Anlagen II, Aktenstück Nr. 63: 144.

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GERMANY'S ELITES—the mayor, the *Landrat*, the Junker, the priest—never thought to allow a plebeian electorate to usurp their traditional roles. Subsequent elections produced even more open attempts at influence by local worthies and more overt interference on the part of government officials than in 1871, a development that some attributed to the spread of "Prussian" conditions. A paternalistic employer might lead his workers to the polls, with music playing. More confrontational ones, such as the directors of the Georg-Marien Hütte and the Osnabrück steelworks in the Fourth Hanoverian district in 1874, posted notices demanding that workers assemble at the plant on election morning and then forced them to listen to speeches warning against opposition voting. Ballots were of course given out in front of the polls. In the Ninth Hanoverian, factory owners simply ordered their staff to vote for their candidate and fired them when they did not.[17] "Things happen in these elections that one would not believe possible in this century and in Germany!" exclaimed one shocked deputy in 1874. Attempts to influence and intimidate the electorate did not diminish over the years. Brett Fairbairn reports that between 1893 and 1898, one in thirty victories was nullified for electoral misconduct.[18] After the 1912 elections, the validity of the outcomes in nearly a quarter of the districts was challenged. Twelve victories were thrown out immediately, and, when war broke out two years later, twenty-eight seats, including those of such luminaries as Ernst Bassermann and Friedrich Naumann, were still under investigation.[19]

By all appearances, it should not have been difficult for the German elites to control elections. Contrary to the assumptions in much of the literature, the integrity of the imperial election system was not protected by the secrecy prescribed in the election law.[20] "By far the majority of the voters are convinced," a deputy noted, "that the chairman of the election board . . . in one way or another perceives or learns to whom [they] are giving their votes."[21] Little was done in subsequent years to change that conviction. In Hoyerswerda, in the westernmost corner of Lower Silesia, the chairman took the ballot from Stap the plowman, held it up to the light and called out, "This one's all right!" (*Es stimmt!*) before putting it in the voting urn. When Grüsche, the gardener's son, arrived, another election official, Estate Owner Douglas, took away his ballot, saying, "I just know *that's* not going to be right" (*Weis' einmal, der ist nicht richtig*) and thrust

[17] Report of Abt. III, March 25, 1874, SBDR, Anlagen I, Aktenstück Nr. 118: 397–99; SBDR, April 10, 1874: 706–12.

[18] Quote from SBDR, April 10, 1874: 708. Excellent studies of government influence include Hans Fenske, "Der Landrat als Wahlmacher: Eine Fallstudie zu den Reichstagswahlen von 1881," *Die Verwaltung*, 4 (1979): 433–56; Brett Fairbairn, "Authority and Democracy: Prussian Officials in the German Elections of 1898 and 1903," *Historical Journal*, 33 (1990): 811–38, statistic on 816.

[19] "Reichstagsschluss und Wahl-Prüfungen," *Berliner Neueste Nachrichten*, May 23, 1914; and untitled articles in the *Vossische Zeitung*, May 13, 1914, and the *Staatsbürger Zeitung*, May 14, 1918, Nr. 112, all in BA Potsdam, Reichsamt des Innern 14653/1, unfoliert. Jürgen Bertram, *Die Wahlen zum Deutschen Reichstag vom Jahre 1912* (Düsseldorf, 1964), 129–38, reports widespread government attempts to influence elections.

[20] See, for example, Steinbach, *Zähmung*, 1: 33. Suval (*Electoral Politics*, 41–42) and Fairbairn ("Authority and Democracy"), in registering but then discounting election misconduct, may simply be regarding the glass that I consider half empty as half full.

[21] SBDR, 1869: 199, cited by Pollmann, *Parlamentarismus*, 326, n. 91.

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one naming a Conservative in his hand instead.[22] In small communities, a man's political position might be known to his neighbors before he even took up a ballot. This was true in the very first election, as shown by the cry in one Upper Bavarian polling place—"And here comes another Pachmayr!" (the Liberal candidate)—when timber dealer Martin Peer walked in the door. That was all it took to make the abashed Peer turn around and go home without voting.[23]

Because ballots were not printed centrally but by the candidates themselves, it was always possible to use a slightly different weight of paper, a slightly creamier shade of white, or to put some distinguishing mark on the outside of the ballot as a means of monitoring the votes. In Conzell in 1874, voters who presented ballots for the Liberal candidate were told by the chairman of the election board, "We can't use that ballot," and (pointing to the pastor, hovering in the back of the room) "go on over there to his Reverence and fetch another."[24] During the hotly contested race in the Ruhr mining town of Bochum in 1881, Center Party workers stayed up the night before the election, cutting the edges off their candidate's ballots so they would resemble the miniature ones the mine owners were distributing.[25] Envelopes to conceal the ballots from prying eyes were finally introduced in 1903, but many complained that these actually made the situation worse. Before, a wary voter could hope that his own ballot paper might get lost among the others tossed in the "urn." The standardized, bulky envelopes encouraged any election chairman worth his salt to use as an "urn" a receptacle, such as a cigar box, small enough to hold them tightly in place. At the end of the polling, the ballots would still be in the order in which they were cast. It was then child's play to correlate the sequence of ballots with the sequence of voters.[26] Year after year, on through 1912, violations of secrecy were both charged and proved.[27]

Even when the ballot was not violated, secrecy was often impossible. Employers found ways to assert political authority over their own workers, even when they could not always change the outcome of the elections. Employers in the Emsland, knowing from experience that they could not defeat the popular Center Party incumbent, Ludwig Windthorst, in the most Catholic district in Germany, hit upon the ploy in 1890 of allowing him to run unopposed—and then required their men to request time off individually if they wanted to vote.[28] And when

[22] Report of Abt. VI, March 9, 1874, SBDR, Anlagen I, Aktenstück Nr. 61: 238.

[23] Report of Abt. V, April 20, 1871, SBDR, Anlagen II, Aktenstück Nr. 38: 97–99.

[24] SBDR, April 11, 1874: 716. Violations were as frequent in Protestant areas (for example, Report of Abt. II, March 19, 1874, SBDR, Anlagen I, Aktenstück Nr. 101: 314–16; SBDR, January 7, 1875: 873); and there was plenty of monitoring by the Left. See Report of Abt. IV, April 5, 1871, SBDR, Anlagen I, Aktenstück Nr. 30: 87.

[25] Julius Bachem: Stenographische Berichte über die Verhandlungen des preussischen Hauses der Abgeordneten, March 3, 1882: 627.

[26] See, for example, the Königsberg scholar, Richard Siegfried, "Die verschweigene Wahlurne," in *George Hirths Annalen den Deutschen Reichs* (Jahrgang 1906): 735–60; also Suval, *Electoral Politics*, 48–50.

[27] See the many examples cited in Guido Leser, "Untersuchungen über das Wahlprüfungsrecht des deutschen Reichstags: Zugleich ein Beitrag zur Frage, Parlamentarische oder richterliche Legitimationsprüfung?" (Dissertation, Heidelberg, 1908), 93 n. 2; Justizrat Dr. Johannes Junck to Geheimrat Jungheim, September 21, 1912, BA Potsdam, Reichstag 3346, Bl. 297–98v.

[28] H. Ramme [Kamme?] to Forst- und Domäneninspektor Rudolf Clauditz, February 20, 1890, Niedersächsische Staatsarchiv, Osnabrück, Dep 62b, 2379. I am indebted to Dr. Joseph Hamacher of Haselünne in Germany for lending me these documents.

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entire mining villages in Hanover's Hameln-Springe that same year unanimously returned the Social Democratic candidate, how could the mining director not know how each man had voted?[29]

CONSIDERING THE SLENDER CHANCES FOR SECRECY, and especially considering all that has been written since about the effectiveness of Germany's authoritarian institutions, the anti-government results of imperial elections should come as something of a surprise.[30] The share of seats belonging to those parties whose victory the government actively promoted began with a modest 57 percent in 1871, a figure that is even less impressive when one remembers that the polling took place the very day after peace was signed in a hugely successful war: that is, after twenty-four hours of euphoric bell ringing, hymn singing, and torch-light parades.[31] The pro-government share sank more or less continuously to a dismal 25 percent in the thirteenth general election in 1912, the last before World War I.[32] Of the three districts mentioned at the beginning of this essay, Oberhaid in Bavaria never returned a candidate favored by the government, Einbeck in Hanover veered back and forth between governmental and oppositional candidates until 1912 and then returned a Social Democrat, and the Potsdam district was a Socialist bastion for the last six elections before the war.

So the historian is faced with a puzzle. How were the old authorities forced to make room for new voices? How in the bureaucratic *Obrigkeitsstaat*—a state based on authority and hierarchy—did the opposition, or oppositions, succeed in expressing choices so at odds with the powers that be? One answer, of course, is urbanization. What neither Bismarck nor his cronies General Albrecht von Roon and Count Friedrich zu Eulenberg had figured into their calculations in 1867 when they made their wager on democracy was that Germany was about to experience a sudden explosive growth of cities.[33] In the decade between 1880 and 1890, the numbers of German cities with populations over 100,000 increased from fifteen to twenty-six. Supported by the anonymity of the new megalopolis—this argument might run—voters escaped both the meshes of deference and the terrors of officials and employers. Town air makes free. Such cities were simply too large for traditional authorities to control.[34]

There is much plausibility in this argument. Support for oppositional parties

[29] Report of the Commission on Election Scrutiny, June 21, 1890, SBDR, Anlagen I, Aktenstück Nr. 95: 638–42; SBDR, December 3, 1890: 760–66. Otto Hue recounts vividly the "election terrorism" (*Wahlterrorismus*) exercised by mine owners in *Die Bergarbeiter: Historische Darstellung der Bergarbeiter-Verhältnisse von der ältesten bis in die neueste Zeit*, 2 vols. (Stuttgart, 1910–13), 1: 247–48.

[30] Institutionalized authoritarianism is prominent in Hans-Ulrich Wehler's widely used textbook, *The German Empire, 1871–1918* ([*Das Deutsche Kaiserreich*, Göttingen, 1973], New York, 1985).

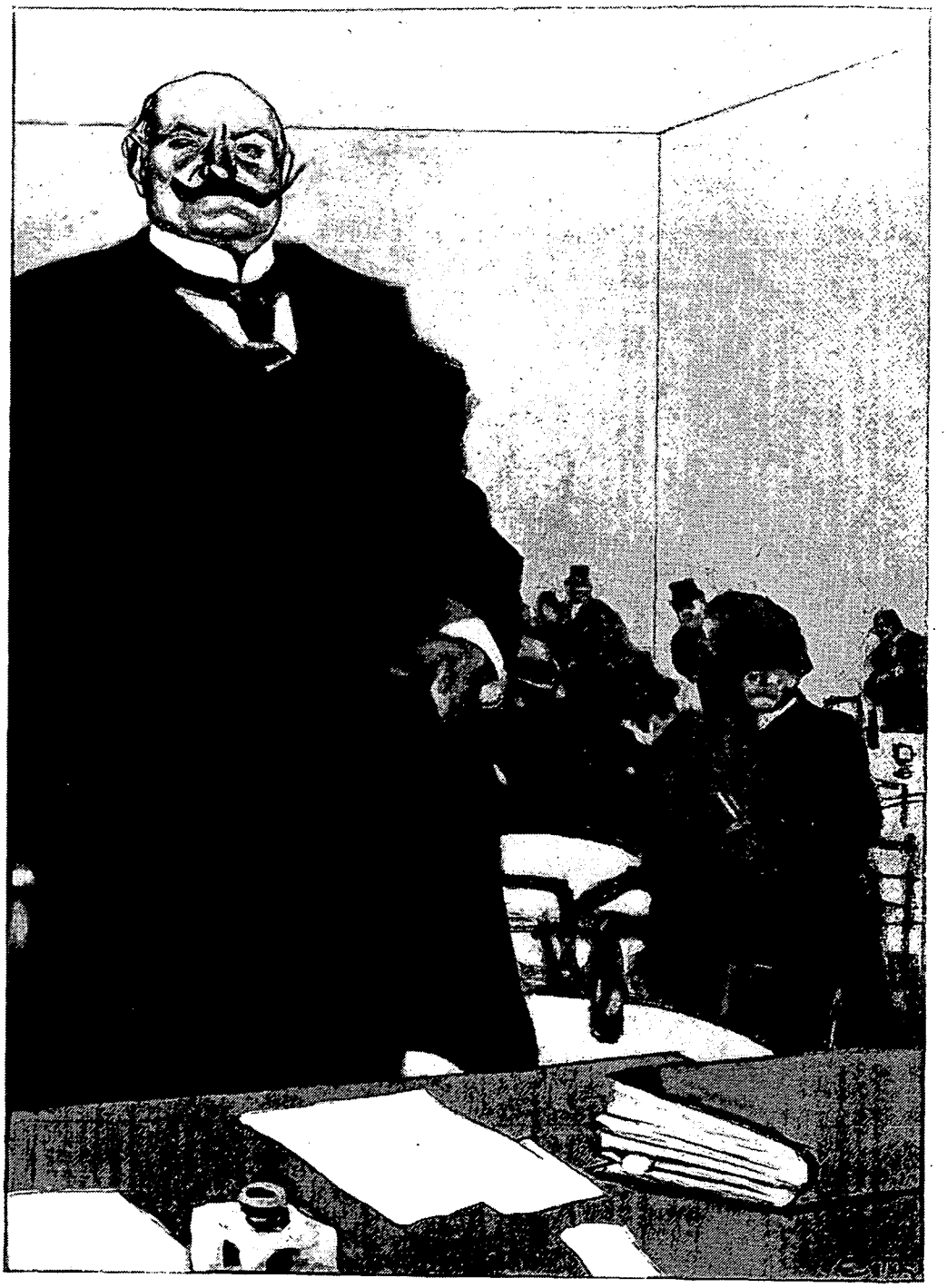
[31] Numerous notices of local celebrations appear in the *Görlitzer Anzeiger* and the *Hallische Zeitung*, March 3, 4, 5, 1871.

[32] These are round figures for seats; the pro-government vote is even lower.

[33] For Bismarck's motives, see Ernst Richard Augst, *Bismarcks Stellung zum parlamentarischen Wahlrecht bis 1871* (Leipzig, 1917); and, more recently, Theodore Hamerow, "The Origins of Mass Politics in Germany 1866–67," in Imanuel Geiss and Bernd Jurgen Wendt, eds., *Deutschland in der Weltpolitik des 19. und 20. Jahrhunderts: Fritz Fischer zum 65. Geburtstag* (Düsseldorf, 1973): 105–20; and Pollmann, *Parlamentarismus*, 68–79.

[34] Steinbach makes a similar argument in *Zähmung*, 1: 35.

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The Result: "So, gentlemen, the people have spoken—now it's His Majesty's turn to speak for another five years." From *Simplicissimus*, 16, no. 2 (1911–12): 736. Artist: Eduard Thöny.

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correlates positively with the size of cities. Yet urbanization cannot be the whole story. Most Reichstag deputies were not elected by the big cities. A metropolis was in any case not where most Germans made their home. In 1890, after two decades of unprecedented demographic growth, nearly 60 percent of Germany's citizens still lived in towns and villages of fewer than two thousand residents. As late as 1905, after the empire's total population had passed the sixty million mark, well over half of these people lived in communities under five thousand.[35] But more important than any statistic is the fact that large urban electorates do not of themselves disable a government determined to control voting; mockeries of free elections were common in New York City in the Gilded Age and, indeed, in many cities today. The urbanization argument contains an unexpressed—and rarely acknowledged—premise: there was a certain level of interference beyond which German authorities would not go. Wholesale destruction of ballots was beyond the pale; violence against voters was inconceivable. That the authorities were unwilling to embark on either of these courses is itself an important observation about the elites of imperial Germany.

Another, more complicated solution to the puzzle of oppositional voting in an authoritarian society has been found in the growth of large, national political parties. Eventually, such parties developed a power locally that might rival the authority of a Junker, mayor, *Landrat*, or priest. The best-known example here is the Social Democratic Party, about which there is a wealth of literature. But Socialist party identity was built up and legitimated by a web of nonpolitical social relationships, which took time to evolve.[36] Moreover, to cite the legitimacy of parties as rival authorities to notables and officials as an explanation for the successful expression of political opposition begs the question being raised here, for party development itself presupposes an openness in the system that is precisely what needs to be discovered.

Consequently, in what follows I shall examine not the party development of the future—the 1880s and beyond—nor indeed any of Germany's new developments and arrangements. Rather, I will look at those features of the traditional order, already within German culture, that provided the early handholds, the rough crevices in the smooth system of authority, which allowed some groups of voters as early as the 1870s to gain a purchase on the wall of *Obrigkeit*. Two such handholds were rules and religion. The election in Oberhaid showed how local authorities—Mayor Fösl and Royal Pastor Keck—responded to a new institution with old expectations, based on traditional deferential politics. We shall now look at some of the new voices that succeeded in making themselves heard by using old habits, assumptions, and attitudes.

GERMANS TALK ABOUT A "Rechtssinn," a "sense of justice," but I wish to call attention to the more specific—and for that reason perhaps more powerful—"Gesetzsinn,"

[35] *Statistisches Jahrbuch für das Deutsche Reich* (Berlin, 1893), 1; *ibid.* (Berlin, 1908), 6.

[36] Brilliantly described by Paul Göhre, *Drei Monate Fabrikarbeiter und Handwerksbursche* (Leipzig, 1891); in English as *Three Months in a Workshop: A Practical Study*, A. B. Carr, trans. (London, 1895), 97, 104, 106–07, 193–94.

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a sense that written laws (at least their letter) and regulations should be obeyed. This aspect of German political culture goes back very far. Winfried Schulze has noted a tradition of the "judicialization of social conflict" in the eighteenth century, even at the village level, as a peculiarly German phenomenon.[37]

If there were such a "Gesetzsinn" in electoral politics, we would expect to see it first of all in the behavior of officials. And we do, as early as the election of 1871. In the town of Moers near Düsseldorf, for example, an alert police officer, Rohkoch, noticed that the chairman of the election board, van Laak, was late for the start of the elections, thus depriving the board of the legally stipulated quorum. He immediately filed a charge against Chairman van Laak with Mayor van Laak. Unfortunately, Chairman van Laak and Mayor van Laak were one and the same. No matter. Officer Rohkoch knew where authority, and hence where duty, lay.[38] It lay not in persons but in the rules themselves. Officer Rohkoch's civic courage was not a function of any liberal self-assurance in his rights as a citizen but a function of his faith in authority: the authority of the written regulation and the authority of his own job to enforce it.

Such a faith in written rules manifested itself in a number of other ways. For example, although the Reichstag, when deliberating on the new voting regulations, decided that the public interest required that civil servants not be allowed to sit on local election boards, the deputies continued to entrust state officials with the countywide tallying of returns. Their anxiety about civil servants was clearly limited to the danger of improper "influence" on the voters.[39] That the government might also influence an election by a dishonest count, demonstrated by contemporary practice in Italy and Spain (and as late as the May 1989 municipal elections in the German Democratic Republic), was evidently inconceivable.

The conviction that rules must be obeyed, something often cited as a fatal weakness in the German political character, making Germany vulnerable to authoritarianism, could have unexpected subversive consequences.[40] The law required, for instance, that the election codes be put on display in each polling place. Voters studied these documents carefully in order to find ways to counteract the intimidating authority, both tacit and overt, of officials, notables, and "bread lords." Because it cost nothing in Germany for a citizen to lodge an election protest, many did. Unlike Britain, where such challenges were a private legal matter and, even after reforms in 1868, cost the plaintiff a minimum of £1,000, in Germany the Reichstag itself, upon receiving a petition that established

[37] Winfried Schulze, "Peasant Resistance and Politicization in Eighteenth-Century Germany," paper delivered at the American Historical Association annual meeting, December 1987; and Schulze, *Bäuerliche Widerstand und feudale Herrschaft in der frühen Neuzeit* (Stuttgart-Bad Cannstatt, 1980), 141.

[38] Report of Abteilung VI on the Seventh Düsseldorf (Moers-Rees) election district, SBDR, Anlagen II, Aktenstück Nr. 10: 16; SBDR, March 31, 1871: 77.

[39] Hatschek, *Kommentar*, 170–79.

[40] Reinhart Koselleck has demonstrated this *Paradox des Gesetzwerkes*, the ability of the written law to subvert the claims of the traditional social order that (some would say) it is supposed to protect, for an earlier period in "Staat und Gesellschaft in Preussen 1815–1848," in Werner Conze, ed., *Staat und Gesellschaft im deutschen Vormärz 1815–1848* (Stuttgart, 1970), 79–112; and especially in Reinhart Koselleck, *Preussen zwischen Reform und Revolution: Allgemeines Landrecht, Verwaltung und soziale Bewegung von 1791 bis 1848* (Stuttgart, 1967).

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a prima facie case of malfeasance, could require the state to conduct an investigation.[41] Then it would review the evidence and make a decision.

At first, these plebeian challengers framed their election protests too loosely, without names and dates, and found them almost invariably dismissed for insufficient evidence. But they quickly learned from their mistakes. Moreover, they took great advantage of Paragraph 9 of the election law, which provided that election proceedings be open to the public. Catholic priests were the first to stand guard at polling sites, ready to protect their interests by reporting the first infraction or sign of improper influence.[42] By 1874, in the Saxon city of Crimmitschau and in Berlin, Socialist partisans were taking up these positions as well. Polish Party members in Thorn set up a relay system, and whenever one of their own number left the polling place, another sat down to monitor the election in his stead.

Such efforts did not always meet with success. In the Neumünster precinct in Berlin, the election board chased away Socialist poll-watchers with fire hoses. In Thorn, the election officials summoned constables to throw Polish Party activists out, and they eventually shut the polls down before many voters had the opportunity to cast their ballots. On the flimsiest of excuses, both Polish and Socialist poll-watchers might be jailed for weeks.[43]

Those with something to lose tried to interpret the offending Paragraph 9 about public elections as narrowly as possible, in hopes of driving these unwelcome sentries away. They insisted that the word "public" should apply only to eligible voters. When that did not end the surveillance, they tried to restrict "the public" to the more vulnerable local voters, arguing that only those actually empowered to vote in that particular precinct should be admitted inside its polling place: an obvious effort to exclude less dependent outsiders.[44] Although it took decades for the "public" to be defined as broadly as oppositional forces demanded, by the early 1890s, both the Reichstag and the courts had begun consistently to rule in their favor.[45] In 1892, these decisions were made universal through a ministerial decree.[46] Eventually, election officials were denied the authority to demand that someone desiring entry prove first that he was an eligible voter, a German citizen, or even an adult. Rather, the burden of proof was

[41] For Britain, Gash, *Politics in the Age of Peel*, 113, 133–34; William B. Gwyn, *Democracy and the Cost of Politics in Britain* (London, 1962), 83–92; Cornelius O'Leary, *The Elimination of Corrupt Practices in British Elections, 1868–1911* (Oxford, 1962), esp. 47.

[42] Report of Abt. V, April 20, 1871, SBDR, Anlagen II, Aktenstück Nr. 38: 94–95, 98–99.

[43] Testimony of Otto Reimer, Lassallean, and Dr. Anton von Donimirski, Polish Party: SBDR, April 10, 1874: 696, 710.

[44] Reimer: SBDR, April 10, 1874: 697.

[45] Strafsenat des Königlichen Kammergerichts zu Berlin, November 3, 1890, BA Potsdam, Reichstag Nr. 3343, Bl. 176–80v; SBDR, March 18, 1892: 4841; SBDR, 1890–92, Anlagen, Aktenstücke Nrs. 169, 184, and 707; SBDR, 1894, Anlagen, Aktenstück Nr. 217: 1147; SBDR, May 1, 1900: 5174; Hatschek, *Kommentar*, 172.

[46] Ministerial Erlass of July 18, 1892, cited as a warning in *Die Ungiltigkeit von Reichstagsmandaten und deren Verhütung: Rathgeber bei der Abhaltung von Wahl-Versammlungen und Wahlen für den Reichstag*, published by the Wahlverein der Deutschen Conservativen für seine Mitglieder (Berlin, n.d. [1897]), 28. Enforcement can be traced in Memorandum of June 13, 1892, and in Hanic (of the Interior Ministry) to the Imperial Chancellor, July 11, 1892, BA Potsdam, Reichsamt des Innern, Nr. 14668, Bl. 11, 14.

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put on the official to show why that person should not be admitted.[47] By the time of the Weimar Republic, "party checkers" were even given officially designated space in polling places, a sign not just of the legitimacy but of the hegemony of "party" in German political life.[48]

The efficacy of policing the polls did not depend on the Reichstag throwing out an election result upon every credible charge of impropriety. In most cases, for reasons both technical and political, it allowed elections to stand. The deputies' response to misconduct was often what can only be called philosophical; as one member put it, if they overturned every election in which there were problems, there would be no deputies in parliament.[49] The Reichstag was always loath to invalidate an election when the influence charged was exercised privately, which protected most employers. The deputies were tougher with Catholic priests and civil servants. But, in any given instance, their ruling was unpredictable, as one high imperial official, at the end of a sixty-four-page analysis of more than ten years of Reichstag scrutinies, concluded despairingly.[50] Yet, regardless of the outcome of the challenge, the mere presence of oppositional poll-watchers gave courage to citizens of their own persuasion. It also established a system of counter pressures on the voter that was often the mirror image of the pressures traditionally exerted from "above."

Critics deplored this development as "election terrorism" (*Wahlterrorismus*).[51] Terrorism of a sort it may have been; but, in the gentler nineteenth century, terror was induced by a combination of moral authority, social conformity, and, in later decades, the threat of boycott.[52] At worst, the silent surveillance of partisan monitors set limits on blatant intimidation and prevented local election officials from rigging the count. At best, with two powerful forces forming the gauntlet as he walked up to the election urn, the German voter gained some space for himself. Thus the election rules themselves, but particularly the legitimacy traditionally accorded to rules, ultimately even by those in authority, provided a mechanism by which the edifice of official and notable control over elections could be, and was, subverted.

Of course, those German authorities, high and low, who felt that the security of

[47] SBDR, June 13, 1890: 319; SBDR, 1890, Anlagen, Aktenstück Nr. 286: 2. Bundesrat amendments to the election code in 1903 that would have prohibited voters from hovering around the polling place after they had voted never passed the Reichstag: "Entwurf einer Bekanntmachung betreffend Abänderung des Wahlreglements vom 28. Mai 1870," *Reichstags-Wahlrecht, Wahlverfahren, Wahlprüfungen: Zusammenstellung der sämtlichen gesetzlichen Bestimmungen hierüber, nebst den Grundsätzen der Wahlprüfungskommission betreffs der Giltigkeit und Ungiltigkeit von Wahlen u.A.m.* (Berlin, 1903).

[48] The purpose of Weimar party representatives was not to ensure honesty but to make lists of delinquent voters, who could then be fetched. In England, poll-watchers were forced to stand outside. James Kerr Pollock, *German Election Administration* (New York, 1934), 34.

[49] SBDR, April 11, 1874: 718; see also Edward Banks, SBDR, April 10, 1874: 691.

[50] Report by Regierungsrat Heinrich von Poschinger, February 1879, BA Potsdam, Reichsamt des Innern, Nr. 14450.

[51] Wilhelm Löwe-Calbe: SBDR, April 11, 1874: 718; Göhre, *Drei Monate Fabrikarbeiter.* Social Democratic "terrorism" led some conservatives to support a secret ballot for Prussian state elections. Below, *Das parlamentarische Wahlrecht,* 155, 156 n. 129, 167; D. [Delbrück], "Preussische Wahlreform," *Preussische Jahrbücher,* 130, Heft 1 (Oktober bis Dezember 1907): 188–91, esp. 191; Pollmann, *Parlamentarismus,* 85, n. 96, 171, n. 110.

[52] Charges against Social Democrats by the Left Liberals appeared in the *Frankfurter Zeitung* 1908, Nr. 174. Underlying Suval's argument that Wilhelmine elections were basically free is a very positive view of "affirming communities" that tends to wink at community pressures against dissent.

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the country rested in their hands, studied the rules as well. Such was the case with Herr Drewelow, chairman of the election board in a precinct in Marienwerder, bustling to inform his *Landrat*, the Prussian state's chief officer in the county, "on oath" that he had deliberately failed to set out the list of voters in his precinct the week preceding the election, as the law required, "so that, if necessary, the election could be declared invalid." Because the election in Drewelow's district had just been won by the very unwelcome Polish Party, the conscientious Drewelow clearly expected congratulations for his careful dereliction of duty.[53]

Chicanery like this was never lacking. A lengthy survey of election practices by the Reichstag's Standing Committee on Election Oversight at the close of the century revealed that the manner and frequency of election misconduct had not changed significantly since the introduction of democratic suffrage.[54] But the stipulation that the Reichstag itself validate every election and review every alleged infraction provided an invaluable forum, both for the articulation of oppositional interpretations and eventually for their prevalence. Gradually, a body of case law developed that expanded the space for political opposition.

THE CAREFUL STUDY AND EXPLOITATION OF THE RULES was a less effective means of self-assertion, however, as one moved east. The eastern population was less literate, often Polish speaking, and economically more dependent; it was harder to inform voters about the rules and harder yet to convince them to trust them. East of the Elbe, the old authorities proved less willing to allow their traditional prerogatives to be inhibited by newfangled regulations. It took a different kind of handhold to scale the walls of *Obrigkeit*. That purchase might be provided by religion. Take, for example, the campaign in Pless-Rybnik, which, precisely because the political use of religion excited such an uproar, has left a lengthy paper trail in its wake.[55]

In 1871, a well-known incumbent was defeated by an outsider and complete unknown: Father Eduard Müller, mission-vicar in Berlin, whose party, the Center, had been formed to protect Catholic interests in the new German empire. For Liberals, Progressives, and Conservatives, it was axiomatic that a man without a local reputation could never have succeeded but for the influence of unworthy clerical pressures. During the debate on the challenge to this election, the deputies waxed indignant at the temerity of Father Müller's supporters in campaigning on the platform that the church was in danger from rising anticlericalism, when Müller's opponent, a Free Conservative, was himself a Catholic. Even greater umbrage was expressed on hearing that Müller's supporters had made an issue of their candidate's asceticism, depicting him "approximately the way Upper Silesians would like to conceive of their saints, as a man who . . . lives a kind of hermit's life in anachronistic simplicity." Those in the Reichstag chamber snick-

[53] SBDR, April 11, 1874: 736–67.

[54] Bericht der Wahlprüfungs-Kommission über die Ergebnisse der Wahlprüfungen in der neunten Legislaturperiode von 1893 bis 1898," in *Reichstags-Wahlgesetz vom 31. Mai 1869 und Wahlreglement* . . . (Munich, 1907), 77–86.

[55] Documentation, and a geographically broader discussion of the use of Catholicism, can be found in my article, "Kulturkampf and the Course of German History," cited above.

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ered to hear that Müller had been lauded as someone who "in the greatest poverty [subsisted] in a dark little room, in the greatest cold has no clothes to put on, and if someone takes pity on him and gives him an old fur, he gives it to the poor. He has no bed, no stove in the room, goes hungry, and dispenses with all human necessities. Already he is a half-saint."[56]

Liberals and Conservatives alike professed dismay at the depths of ignorance that voters must have possessed to respond to a campaign framed around appeals to such medieval values. But all their high-minded clucking avoided mentioning the obvious: Father Müller's life of poverty made a striking contrast to the life of the losing candidate. That unhappy Free Conservative, touted on the Reichstag floor as an incumbent and as himself a distinguished Catholic, was best known locally as the owner of vast estates, both in the election district, where he resided in Castle Rauden, and in the neighboring counties of Gleiwitz, Rosenberg, and Ratibor. The defeated incumbent was Victor Moritz Karl, the duke of Ratibor, the prince of Corvey and prince zu Hohenlohe-Waldenburg-Schillingsfürst, general of the cavalry, brother to Prince Chlodwig zu Hohenlohe-Schillingsfürst (lately minister-president of Bavaria and future chancellor of the empire), and, as *Standesherr* of one of the oldest families in Germany, a man equal in rank to the king.

Shivering priest, glittering duke: the contrast in wealth and status mirrored the great social distances within Upper Silesia itself. Upper Silesia was the land of the super rich. The very term "Prussian magnate," as Theodor Fontane would later observe, implied "Silesian."[57] Farming the estates and working the mines of these lords of creation was the poorest population anywhere in Germany. The principality of Pless, which together with the county of Rybnik made up the disputed election district, was the most impoverished constituency of them all. These facts, too, the Reichstag's indignant majority passed over in silence.

Consequently, the deputies left unmentioned where the poison in the rhetoric of poverty actually lay: in its invitation to the voters to see themselves reflected in the Center Party champion. A candidate who shared a life of poverty, even if he was a priest and a Berliner, was a man with whom they could "identify." To encourage a man to support someone with whom he can identify over a well-born rival may indeed be demagogic; it is also an inevitable feature of democratic politics and a deeply subversive one. Once the criterion for public office shifts from superiority over the voter to identification with him, the foundation of a politics of deference disappears.

But the significance of the duke's defeat by the lowliest of Berlin priests is not exhausted by referring to the onset of a new politics of identification. The duke of Ratibor had also been the candidate of the prince of Pless, the man who virtually owned Pless County (half the election district), together with its coal mines, its timber, and most of its land. He also controlled its police power.[58] In

[56] Report of Abt. III, November 12, 1871, SBDR, Anlagen II, Aktenstück Nr. 69: 164–69. More coherent and more ludicrous versions, respectively, are in the *Hallische Zeitung*, 4th Beilage zu No. 103, May 3, 1871, and in the *Görlitzer Anzeiger*, Nr. 103, May 3, 1871.

[57] Theodor Fontane, *Der Stechlin* ([1898] Zurich, 1975), 169.

[58] Hans Heinrich XI, Fürst von Pless, Graf von Hochberg, Freiherr zu Fürstenstein, Oberjäger-

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1871, the prince had mobilized his entire domainial "interest"—some hundred foresters, his pit foremen, his constabulary—all on the duke of Ratibor's behalf. His constables had gone up and down the streets with drums, making sure everyone knew who the prince's favorite was. His other officials had the finer task of "working" the district with promises—of cash infusions to the widows' fund, for example—and threats—such as an end to wood-gathering rights. To no avail. The prince's man was defeated.

Father Müller's victory in Pless-Rybnik was a straw in the Upper Silesian wind. In that first national election, ten of the eleven other election districts in the region returned conspicuous representatives of the power structure, all belonging to the two Conservative parties: seven counts, two princes, and one cabinet minister. But, by the next election, in 1874, all but one of the twelve districts in this heavily Catholic prefecture had become strongholds of the oppositional Center Party. Five of these seats, moreover, had been taken by someone with a lower rank than the previous incumbent, three by commoners. By 1893, all of the Center's seats in Upper Silesia were held by commoners.

These were not "confessional" victories in the usual sense, for they were not provoked by religious rivalry. Seven of the Conservative magnates who were replaced by Center Party deputies shared the victors' Catholicism. Nevertheless, it was religion—its rhetoric and its leadership—that provided for Catholic underdogs in Upper Silesia the emancipatory tool kit that rules might supply in other milieus: a knife to cut the existing vertical lines of authority and the thread to weave horizontal lines of solidarity. Not least, religion, like rules, provided voters with civic courage, the gumption—of which Germans are traditionally said to be in such short supply—to stand up for one's rights, human and civil, against authority.

But, if these election contests in Upper Silesia were really a struggle for "social emancipation," as one Center Party deputy was pleased to put it, why choose a religious idiom, the "rights" of the church, as the means of expressing it?[59] The first answer would be that it was natural. Traditional Christian *topoi*, the virtues of poverty and humility, provided a ready-made vehicle for the self-assertion of cotters and coal miners vis-à-vis the regnant rich and wellborn. It was natural for the language of Christ to precede the language of class. A second answer would be to concede that the humble of Pless-Rybnik might well have preferred to challenge their betters in the language of social democracy or of Polish nationalism—the population of the district was overwhelmingly Polish speaking.[60] Although the Center Party's eventual losses in Upper Silesia to five Polish Nationalists may imply that possibility, the Nationalist breakthrough occurred only in 1903 and with the slenderest of victories, which suggests that these seats were lost by the policies of nationalist Germans (those of the Center as well as those of the government) as much as they were won by the blandishments of nationalist

meister, owner and resident of the Fürstentum Pless. Paul Weber, *Die Polen in Oberschlesien: Eine Statistische Untersuchung* (Berlin, 1913), 22.

[59] Center spokesman: SBDR, November 22, 1871: 437.

[60] In 1867, 86.6 percent and 92.7 percent of the residents of Pless and Rybnik counties, respectively, were Polish speakers. Weber, *Die Polen*, 8–9.

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Poles.[61] As a practical alternative to Father Müller, however, the election of a National (Polish) Democrat or a Social Democrat in Upper Silesia would have been next to impossible in the first decades of the empire. In the rural East, how could such candidates have reached the population? Open-air meetings required police permission until the reform of the Association Law in 1908. Any tavern that provided rooms for indoor politicking mortgaged its future to police harassment.[62] A combination of No Trespassing signs and watchdogs quarantined the villagers on rural estates.[63] Given that ballots had to be obtained from the candidates themselves or from their agents, it was often physically impossible for a poor man to vote for anyone but the squire's choice.

But not if the clergy intervened. The parish priest, however suspect he might be to the government and the Junkers, had unlimited access to the population that neither watchdogs nor No Trespassing signs could deny. It was no accident that when the Polish National Democrats finally did wrest five seats from the Center in Upper Silesia, three of them were taken by priests.[64]

WHEN IT DEBATED THE CHALLENGE TO THE PLESS-RYBNIK ELECTION, the Reichstag interpreted the result as a victory for the "clerical" program of the Center Party, which in some general sense it was. But party organization was almost nonexistent in Upper Silesia in 1871. Local conflicts were far more important than national programs and issues to the Pless-Rybnik election. Paradoxically, however, the election could reflect local conflicts precisely because it was national and universal. Archimedes said to King Hieron, "Give me a place to stand, and I will move the earth." The national democratic election process established that Archimedean point outside the local universe and provided a lever to break open the closed system that was the village, the county, and even, in some cases still, the town, and release the previously submerged conflicts and desires of their inhabitants.[65] It did this in three ways.

On the most limited level, as in the village of Oberhaid, democratic elections could reveal and set into motion competing deferential worlds: in this case, that of the Bavarian state (represented indirectly by its agitating postmen, forest wardens, and commons keepers, and more directly by Mayor Fösl) versus that of the shepherd of a previously rather isolated local flock: Pastor Keck. As long as

[61] August Hermann Leugers-Scherzberg, *Felix Porsch, 1853–1930: Politik für katholische Interessen in Kaiserreich und Republik* (Mainz, 1990), chaps. 5–8.

[62] See the complaint of cabinetmakers Otto Huth, Karl Kleemann, and Julius Longer to the Reichstag, May 1990, that of the 300 villages and ten towns in their district (Seventh Merseburg), they could hold meetings in only five places. BA Potsdam, Reichstag, Nr. 3343, Bl. 199–200v.

[63] Helmuth von Gerlach, *Vom Rechts bis Links* (Zurich, 1937), 32–33, 156, 161; *Die Ungiltigkeit,* 10 n. 2.

[64] Ilse Schwidetzky, *Die polnische Wahlbewegung in Oberschlesien* (Breslau, 1934), 78–79. That ethnicity was less powerful than confession in determining a constituency's ability to shake off Conservative domination is suggested by the fact that the Center's sweep in Upper Silesia in 1874 included four districts whose Polish-speaking population was 10 percent or less, while the one remaining holdout after 1877, the First Oppeln (which left the Conservative camp only once during the empire), contained a population well over 50 percent Polish but with a sizable Protestant minority (40 percent in 1895).

[65] Krefeld is an example of the latter. See my "Kulturkampf."

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these two deferential worlds had overlapped—or could be thought to overlap—their differences had been invisible and they had reinforced each other. Incipient oppositional forces had little room to develop.[66] But even allegedly passive Germans will take advantage of the space created when worlds collide.

Second, and more obvious, these elections told voters that they were part of a larger process. Voters saw themselves, not least because of the outsiders who came in both to campaign and to patrol the polling places, as empowered by connections—of law, of confession, of class—that reached far beyond their own, relatively powerless, lives. It is true that, by and large, elections offered horizontal in exchange for vertical solidarities: they did not usher in the heyday of the free-floating individualistic voter—if such a person even existed in the nineteenth and early twentieth centuries.

Third, and just as important, the elections gave voters a safe vocabulary with which to articulate their own community relationships. The election lexicon was often limited to the merest slogans—"the church is in danger"; "Father Müller sleeps in the cold." Rarely did this vocabulary give back a precise reflection of the local conflicts it expressed. The struggle in Pless-Rybnik bore only the most distant relation to the values of asceticism or even to the constitutional needs of the Catholic church. But, as the Pless-Rybnik example shows, it is for this very reason—its inaccuracy as a mirror—that campaign rhetoric served to legitimate and therefore bring out into the open conflicts, even class conflicts, that might otherwise have remained submerged—or been expressed inarticulately or violently.[67] A man might feel secure commending his candidate for the pious virtues of poverty and simplicity even if such a commendation was patently a jibe not only at his opponent but at the entire local power structure. The voter's sense of safety, his sense that election rhetoric somehow did not "count," might well be illusory—as the prince of Pless's swift retribution after the 1871 election demonstrated. (He cut off the villagers' poor relief.) But, by then, the break with deference had been accomplished.

It should not be surprising that the words Germans used in their elections were taken from established idioms, for the beginner, as Marx noted, "who has learnt a new language . . . translates it back into his mother tongue."[68] Nor should we wonder that those oppositional voters who pored over the fine print in the election codes and then called on the institutions of the state to enforce them seemed to be conjuring up the "spirits of the past to their service." But that they saw and seized the opportunities to do so is evidence that, in the wake of the domestic and international earthquakes of the late 1860s and early 1870s, Germany's political culture was in considerable flux.

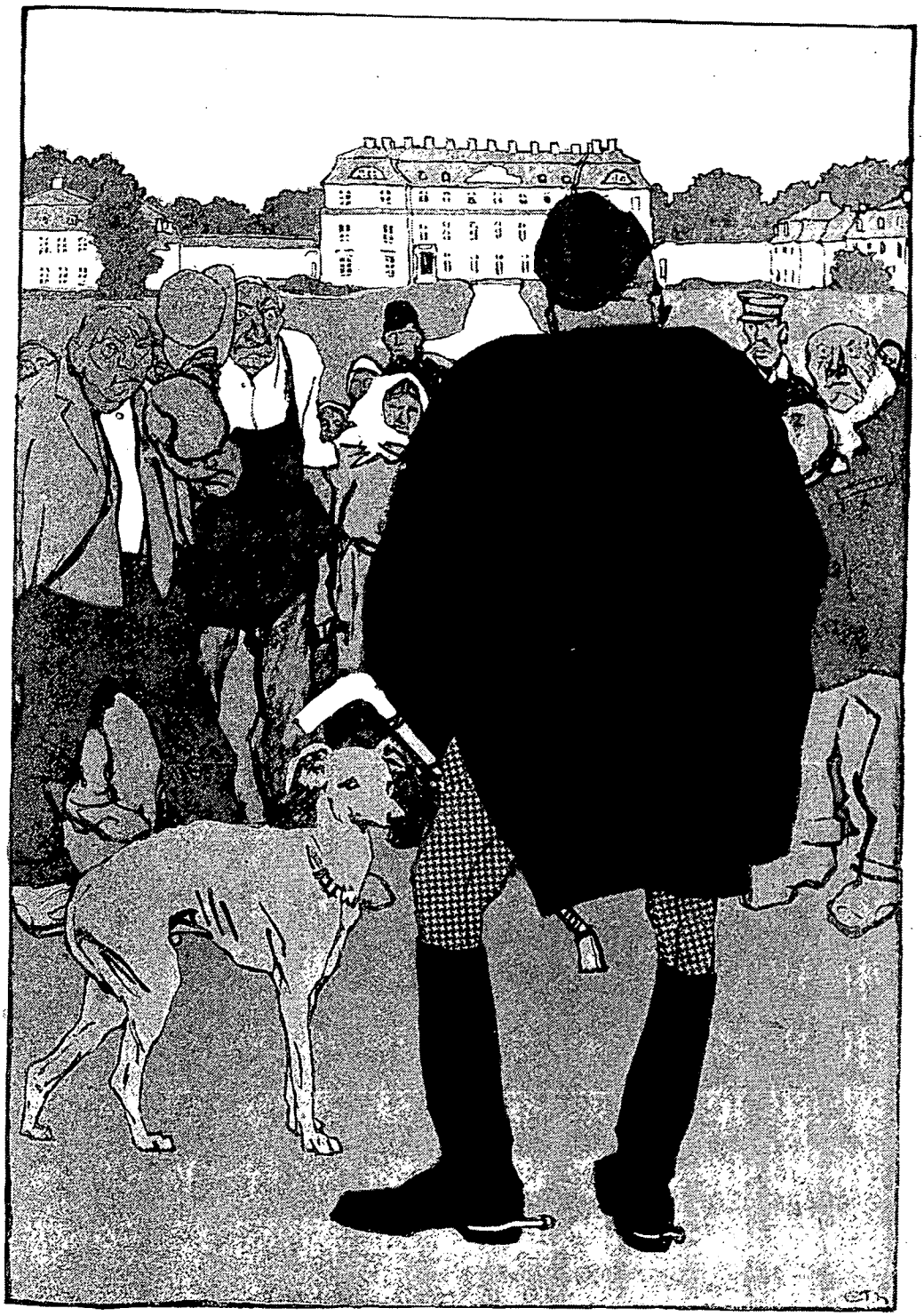
This fluidity is difficult to reconcile with the standard narrative of German

[66] Which may explain why Protestant pastors, representing the majority and drawn from the same social classes as other notables, never became the independent political forces that Catholic priests did.

[67] That Upper Silesia was a tinderbox was demonstrated the summer after the election, when rioting among the miners in nearby Königshütte brought the imposition of martial law for more than eight weeks. See *Görlitzer Anzeiger*, Nrs. 149–52, 154–55, 163, 168, 189, 195, 204, extending from June 29 to September 1, 1871.

[68] *The Eighteenth Brumaire of Louis Bonaparte* (1852), in *Karl Marx and Friedrich Engels, Selected Works in One Volume* (New York, 1969), 97.

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East Elbia: "There was *one* Liberal vote cast. From today on, the schoolmaster gets no more potatoes." From *Simplicissimus*, 16, no. 2 (1911–12): 715. Artist: Eduard Thöny.

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history. That narrative selects for emphasis certain features of "traditional" Germany (including disparagement of politics and deference toward authority) and traces their persistence past the founding of the empire, past World War I, and on to at least 1945.[69] Although important aspects have come under considerable attack in recent years, the hold of the narrative itself remains strong, especially in Germany.[70] Yet, while the behavior of Germans at election time confirms the outlines of the traditional story with which we have become so familiar, it gives that story a different content. Germans frequently did see themselves as apolitical—even as they were expressing a passionate interest in the outcomes of their political choices. Their disinterest in politics is enshrined in their writings and public statements—those of Pastor Keck's, for example. Their interest, on the other hand, is demonstrated by their watchfulness at the polls, as I have suggested above, and by their enthusiasm for voting.

High turnouts can be taken as a marker gene for political mobilization. If we look at national averages, we see an increase in every decade after the introduction of the new franchise. But averages mask the fact that mobilization was far from uniform across regions and social groups. We need to visualize the spread of political mobilization not as advancing waves of invading infantrymen but as parachutists, dropping behind the lines—to borrow a metaphor from Sidney Pollard's account of industrialization.[71] This view of mobilization allows us to see different sets of Germans becoming politically active at different times. Catholics, a good third of the population, entered the political process as a group in the 1870s under the pressure of liberal and government attacks—the *Kulturkampf*. Their turnouts in 1874 averaged over 70 percent and reached 90 percent in some areas. For Social Democrats, the breakthrough came sometime between 1887 and the election of March 1890, while the Socialist Law was still in effect, when their share of the vote increased 95 percent: a jump unequaled by themselves or any other party at any other period in the empire.[72] These two examples of sharp discontinuous change suggest what we already intuitively know: that political identities are not formed by such continuous and relatively universal processes as urbanization and growth but dialectically, that is, in opposition.

Furthermore, a look at Germans as a whole reveals that the most significant

[69] The classic formulation was the sociologist Ralf Dahrendorf's *Society and Democracy in Germany* ([German, 1965], New York, 1967), an ingathering of concepts that had been put forward over several decades by German, North American, and British scholars emphasizing Germany's long-term divergence from "Western" patterns (a divergence subsequently given the convenient label *Sonderweg*—peculiar path). After Dahrendorf, the most prominent proponents of negative "continuities" in German history have probably been Fritz Fischer and Hans-Ulrich Wehler.

[70] The most massive onslaught is David Blackbourn and Geoff Eley's brilliant *Mythen deutscher Geschichtsschreibung: Die gescheiterte bürgerliche Revolution von 1848* (Frankfurt am Main, 1980). A slightly toned-down version was published in New York in 1984 as *The Peculiarities of German History*.

[71] Sidney Pollard, *The Peaceful Conquest: The Industrialization of Europe, 1760–1970* (New York, 1981).

[72] I am discounting the SPD's increase from 1871 to 1874—from 3.2 percent to 6.8 percent—as too small to constitute a breakthrough. Percentages have been calculated from the data in Gerhard A. Ritter and Merith Niehuss, *Wahlgeschichtliches Arbeitsbuch: Materialen zur Statistik des Kaiserreichs 1871–1918* (Munich, 1980), 38–42, 100. See also Johannes Schauff, *Das Wahlverhalten der deutschen Katholiken im Kaiserreich und in der Weimarer Republik: Untersuchungen aus dem Jahre 1928* (1928; Mainz, 1975).

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break in continuity occurred not, as is sometimes asserted, during the 1890s but during the 1870s, whose last election registered a 24 percent increase in turnout over the first.[73] If the plebiscitary election of 1887 is excluded (and it is clearly an outlier, since its totals were not reached again for another twenty years), the biggest sustained jump in national participation in any one election then occurred between 1871 and 1874—an increase of 19.6 percent in a three-year period: yet another sign of the turbulent atmosphere at the empire's founding.

Germans were "political" not only on election day. To a startling degree, they entered into the activities of political parties. Activists will always be a minority in any population, but already by the 1880s a larger proportion of Düsseldorf's electorate were members of a political party than in that same city a hundred years later.[74] Like voter turnout, the propensity toward activism was uneven across regions and groups. But, by 1912, the political scientist Ludwig Bergsträsser was marveling at the "great sacrifices in time and money" that the modern German political party, "that strictly centralized, extraordinarily active giant organism," was demanding and receiving from its adherents.[75]

Far from being the heel-clickers often depicted in the literature (not least in their own literature), these politicized Germans used their voting choices to express a critical view of their rulers and of their society.[76] Their critical outlook is revealed not only by the larger pattern of their electoral behavior: their support for the oppositional Social Democratic, Center, and Left Liberal parties. It is continuously visible also on the micro level, in the precinct.[77] But Germans made their political choices in a world hedged about with controls. The Socialist Law

[73] That the 1890s were a watershed in politicization has been repeatedly postulated by Geoff Eley, most boldly in "Notable Politics, the Crisis of German Liberalism, and the Electoral Transition of the 1890s," in Konrad H. Jarausch and Larry Eugene Jones, *In Search of a Liberal Germany: Studies in the History of German Liberalism from 1789 to the Present* (New York, 1990), 187–216. The 1890s are an odd place to discover a "striking upsurge of popular mobilization" (192), "measured simply by the expansion of the electorate . . . including rising rates of participation" (196), when "the masses dramatically entered politics" (210), registering "a fundamental change in the scale and intensity of political life, in the levels of popular participation . . . articulated in and around the electoral process" (211). For the 1890s saw the smallest average increase in voting participation of any decade in the empire: 9 percent over the average for the 1880s. Participation actually declined from 1890 to 1898. In the three Hessian districts Eley singles out as emblematic of rural mobilization, turnouts were likewise dramatically lower in 1898 than in 1893 and 1890. See Fritz Specht and Paul Schwabe, *Die Reichstagswahlen 1867–1907* (Berlin, 1908), 260–61, 264. Admittedly, nothing as prosaic as turnout statistics will refute the evanescent claim that politics had been "reconstituted" (211, 212), that the "political temperature" rose (200), that the decade was a "vital moment" between "two distinct electoral 'systems'" (193). But, as with those other watersheds in Germany history that were once fashionable, 1866 and 1879, this "absolutely crucial turning point" (202) proves to have been crucial mainly for those perennial protagonists of the German narrative—the Liberals.

[74] Schlossmacher, *Düsseldorf im Bismarckreich: Politik und Wahlen, Parteien und Vereine* (Düsseldorf, 1985), 253.

[75] "Zur Geschichte der Parteipolitischen Agitation und Organisation in Deutschland," in *Vergangenheit und Gegenwart: Zeitschrift für den Geschichts-Unterricht und Staatsbürgerliche Erziehung in allen Schulgattungen*, 1912, Heft 4: 241–53, quote on 252–53.

[76] For heel-clickers, see Heinrich Mann's *Der Untertan* ([1918], in English: *Man of Straw*, London, 1946).

[77] See the petition of dissenting Essen coal miners, written in awkward, ungrammatical language, about the confiscation of their ballots, admittedly too few to affect the outcome of the election: Report of the Committee on Election Oversight, SBDR, March 4, 1891, Anlagen I, Aktenstück Nr. 335: 2181. These men were *not* Social Democrats—whose protests were considerably more skilled.

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(1878–1890) is the example most prominent in the literature, although its practical efficacy has been rather overrated.[78] More revealing were the constant intrusions of bureaucratic authority into every aspect of life—which began long before the anti-Socialist legislation and continued long after.[79] Every German had to register with the police upon taking up even temporary residence in a town. The authorities had to be notified twenty-four hours in advance of every public meeting so that the police could come and take notes. German literature, from Theodor Fontane's *Der Stechlin* (1898) to Paul Göhre's account of his labors in Chemnitz (1891), is full of references, satirical as well as complaisant, to the ubiquity of the policeman, especially, but not only, at election time.[80] German district presidents spent a large part of their time relaying detailed police reports to their superiors. Their correspondence is reminiscent of a CIA station chief operating in a foreign country. In perhaps no other state in Europe were the authorities so well informed of every activity of their citizens everywhere. Everything was "kontrolliert." Although not yet the blanket surveillance ("flachendeckende Überwachung") to which the State Security Ministry of the unlamented East German Republic so recently aspired, in a literal sense, imperial Germany was a police state.

But the word "kontrolliert" in German means "monitored," not "controlled." Imperial Germany was also a state of laws and rules, rules that not only the police but even the ministries eventually had to obey. A public opinion insistent on the letter of the law could put significant limits on what the government could get away with. In 1895, for example, public outrage at police raids against the Berlin Social Democratic Party resulted in the dismissal of a cabinet minister—a man appointed by the kaiser for his determination to get tough with the Socialists.[81]

No less revealing than the *causes célèbres* in which the imperial authorities faced their most radical critics were the small, uncelebrated instances of individuals demanding a hearing from mighty institutions and, within admittedly narrow limits, holding autocratic feet to the fire. The case of the Mecklenburg gardener, August Brandt, is one example. Having moved from Klein-Grabow to Güstrow shortly before the hotly contested election of 1878, Brandt was shocked to find that, when he returned to his old village on election day, the estate owner, Behn, in his capacity as mayor, had struck Brandt's name from the voting rolls. Prevented from casting his ballot, the furious gardener challenged the validity of the election. Nowhere was the social inferiority of a gardener to an estate

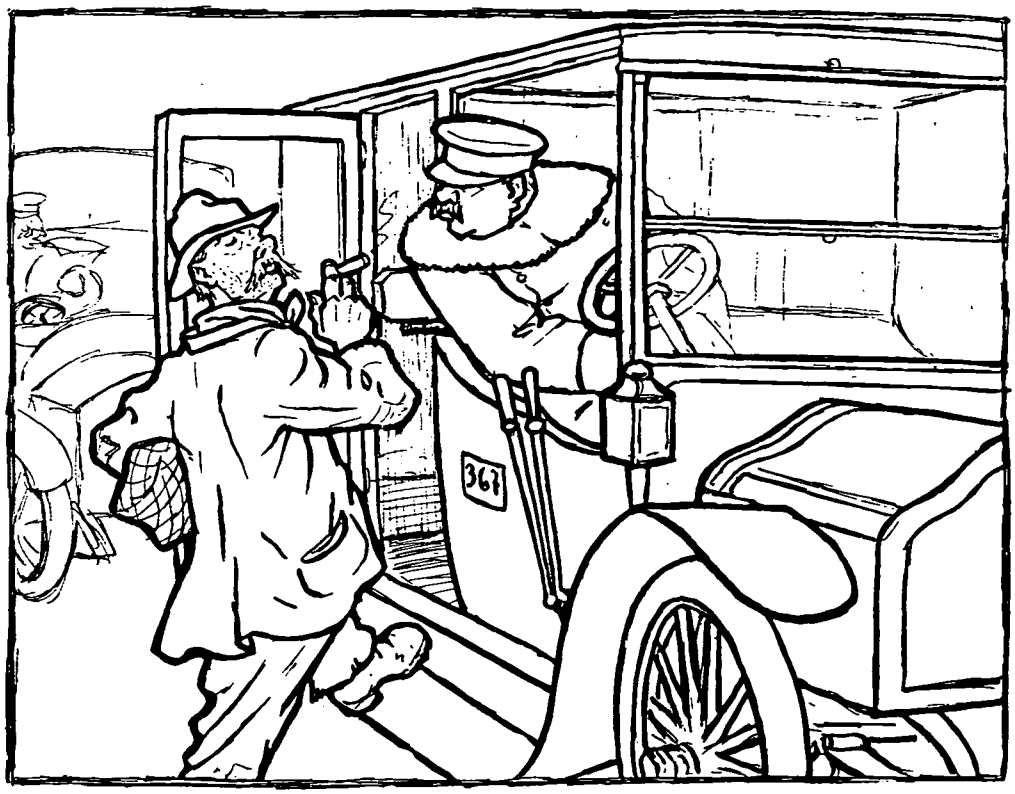
[78] Klaus Tenfelde, *Sozialgeschichte der Bergarbeiterschaft an der Ruhr im 19. Jahrhundert*, 2d edn. (Bonn, 1981), 531 n. 199.

[79] See reports on repression and harassment of Socialists in 1871 in Brunswick (*Hallische Zeitung*, 1. Beilage zu Nr. 104, May 4, 1871), in Glauchau (Saxony), and in Berlin (*Görlitzer Anzeiger*, Nr. 160, July 12, 1871: 1443).

[80] Fontane, *Der Stechlin*, 228–30, 262; Göhre, *Drei Monate Fabrikarbeiter*, 100.

[81] Eleanor L. Turk, "The Berlin Socialist Trials of 1896: An Examination of Civil Liberty in Wilhelmine Germany," *Central European History*, 19 (December 1986): 323–42; esp. 328; for similar scandals during the earlier period, see Vernon Lidtke, *The Outlawed Party* (Princeton, N.J., 1966), 242–44, 273–74; Fairbairn uncovers cabinet-level deliberations to make a similar argument in "Authority and Democracy," 827.

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The Day after the Election: "All *right*, look who's here, Herr Chauffeur! Now you're driving me, and the others gotta go by foot. The Future State begins today." From *Simplicissimus*, 16, no. 2 (1911–12): 735. Artist: Thomas Theodor Heine.

owner—*and* an official—clearer and more punishing than in the semi-feudal Mecklenburgs. But the Reichstag, after reviewing the complaint, found for the gardener and instructed that Behn be officially censured by his state government for his high-handed behavior. The government of the grand duchy of Mecklenburg-Schwerin, after poring over the reports of its own informants as well as the election codes, declined to execute the desired rebuke and through its foreign office informed the Imperial Chancellery of its noncompliance. Thereupon, the bureaucrat in charge, Otto von Bismarck (with what gnashing of teeth we may well imagine) brought the grand duchy to heel by threatening to refuse to defend their aberrant legal interpretation before parliament. The grand duchy was forced to retreat. Although its own legal misgivings had not been entirely removed, it said, "nevertheless under the given circumstances" it would issue the prescribed rebuke. Thus, after being the subject of deliberations by the Reichstag's Standing Committee on Election Oversight and by its 397-member plenum, as well as of extensive correspondence between the president of the Reichstag, the imperial chancellor of the empire, and the government of Mecklenburg-Schwerin extending over four months, August Brandt was granted satisfaction.[82]

[82] The correspondence, extending from October 7, 1878, until February 6, 1879, can be found in

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One is reminded of Michael Kohlhaas, the sixteenth-century horse dealer whose determination to obtain justice forced him to the attention of the royal courts of Saxony, Brandenburg, and eventually of the Holy Roman Empire but with much less happy results. German history does have its peculiar, even "national," continuities. Like the national continuities of every country, they operated in an ever-changing field of force that could produce outcomes capable of startling and chastening those with secure expectations.

BA Potsdam in the records of the Reichsamt des Innern, Specialia, Reichstag Nr. 2, Adhibendum 4, volume 14702, Bl. 1, 3–7, 9.

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Saint-Omer and Its Textile Trades in the Late Middle Ages: A Contribution to the Proto-industrialization Debate

MARCI SORTOR

THE HISTORY OF THE GREAT MEDIEVAL CLOTH-PRODUCING CITIES of Flanders and Artois has long attracted the attention of historians because it touches on a number of themes central to our understanding of both the medieval economy and the emergence of the modern industrial economy. Flanders and Artois comprised the most urbanized region in medieval Europe north of the Alps, and, during the twelfth and thirteenth centuries, the principal cities of this region—Bruges, Ghent, Ypres, Douai, and Lille in Flanders, and Saint-Omer and Arras in Artois—rose to prominence through international trade and the manufacture and sale of woolen textiles. By the late thirteenth century, there were signs that these cities had passed their apogee and were entering a period of prolonged decline. Changes in international trade patterns, especially the decline of the fairs of Champagne, the rise of the German Hanseatic League, and the growing activity of Italian merchants in north European commerce, undermined the position that they had played in long-distance trade. At approximately the same time, these cities sustained a blow to their cloth manufacturing interests from the proliferation of new locations producing cloth for the export market, beginning with the villages and small towns of Flanders and Artois in the late thirteenth and fourteenth centuries, then the villages and cities of Brabant and Holland in the second half of the fourteenth century, and finally those of England in the fifteenth century. These new cloth manufacturers either competed directly with the traditional textile centers by producing similar types of cloth, often at less cost than the traditional producers could manage, or indirectly, by dividing the market for textiles. By the second half of the fifteenth century, the cities that had predominated in Flanders and Artois during the High Middle Ages clearly were in decline: their populations had shrunk, their cloth manufacturing had diminished, and their central position in trade had been lost to new rivals.

The history of these medieval textile centers took on special significance for scholars at the end of the nineteenth century. As northwestern Europe completed

I would like to thank John Munro, Stanley Chodorow, David Ringrose, and Elizabeth Dobbs for their support and comments, and Judith Brown, who read and commented on an earlier version of this argument. I would also like to thank Barbara Hanawalt for giving me an opportunity to present a preliminary version of this article at the 1989 annual meeting of the Medieval Academy of America at Madison, Wisconsin.

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the First Industrial Revolution, which was based on textile production and the building of railroads, historians such as Henri Pirenne turned to the great Flemish and Artésien cloth-manufacturing cities of the Middle Ages to investigate the first traces of industrial production. Pirenne saw a number of parallels between the fate of these cities and the economic developments in late nineteenth and early twentieth-century Belgium. He drew on those parallels to help explain the rise of the textile-manufacturing cities of the High Middle Ages and their subsequent decline.[1] The overlapping of contemporary concerns with research into the past has contributed significantly to the understanding of the economies of these cities, and Pirenne's work remains pathbreaking in many respects. In the case of the decline of medieval urban cloth manufacturing and the rise of textile production in the countryside, however, it has led to a number of false assumptions about the economy of these cities. Indeed, Pirenne and those who adopted his view described the decline of the great medieval cloth-manufacturing cities in terms that evoke the outlook of the economic theories of his day more accurately than they do the actual developments in Flanders and Artois in the later Middle Ages.[2] In particular, the appearance of competition from the countryside when combined with the decline of the medieval urban textile centers led Pirenne to devise a model of two forms of cloth production that pitted city against countryside as representatives of two economies, the one medieval and destined to decline and the other imbued with the attributes that nineteenth-century theorists believed were destined to succeed in the modern economy.

PIRENNE ARGUED THAT THE CLOTH INDUSTRIES of the great medieval cities of this region declined during the fourteenth and fifteenth centuries because rural manufacturing was better suited to the changing circumstances of the late medieval economy than was urban production.[3] According to Pirenne, cloth manufacturing in the countryside was free from the costs of guild membership, fixed wages and hours, and the taxes, seals, and inspections that drove up the price of city-made cloth. Rural manufacturers were also free from the regulations and restrictions that guilds and municipal governments imposed on production in the cities. Additionally, while the medieval cities concentrated on making expensive, luxury woolens, rural cloth production specialized in the manufacture of

[1] Henri Pirenne made the connection between the modern and medieval case in "The Place of the Netherlands in the Economic History of Medieval Europe," *Economic History Review*, ser. 2, 2 (1929): 30, 34–35. This connection has influenced subsequent generations of historians of this area. See, for example, Guy de Poerck, *La draperie médiévale en Flandre et en Artois: Technique et terminologie*, 3 vols. (Bruges, 1951), 1: 10; Hans Van Werveke, "Industrial Growth in the Middle Ages: The Cloth Industry in Flanders," *Economic History Review*, ser. 2, 6 (1954); Myron P. Gutmann, *Toward the Modern Economy: Early Industry in Europe, 1500–1800* (Philadelphia, 1988).

[2] Alain Derville, in his reconsideration of Pirenne's analysis of the history of these cities, makes reference to "postulats libéraux," in "Les draperies flamandes et artésiennes vers 1250–1350," *Revue du Nord*, 54 (1972): 354.

[3] Many of the new "rural" competitors were in fact small towns. They are often classified as rural producers because they retained many rural aspects and often relied as much on rural labor as on their own cloth workers. See, for example, Gutmann, *Toward the Modern Economy*, 42; and David Nicholas, *Town and Countryside: Social, Economic, and Political Tensions in Fourteenth-Century Flanders* (Bruges, 1971), 89.

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both the moderately priced and the inexpensive cloth that became increasingly popular in European markets at the end of the Middle Ages. According to this explanation, the use of indigenous and other wools, which were less expensive than the fine English wool that guilds required urban producers to use, combined with the lower costs of production, allowed rural manufacturers to keep the price of these textiles low.[4] Pirenne's position can be summed up briefly as follows: village cloth makers could be more responsive to changes in market demand and to competition than urban manufacturers, and they could charge less for their products as well. The events of the fourteenth and fifteenth centuries foreshadowed proto-industrialization and the early phases of factory production, both of which took place in the countryside in the eighteenth and early nineteenth centuries. Consequently, the appearance of market-oriented manufacturing in the countryside at the end of the Middle Ages was the inevitable first stage in the development of a modern economy.[5] Pirenne then proceeded to apply theories used to explain the development of the industrial economy of the nineteenth century to the changes taking place in cloth manufacturing in Flanders and Artois between the fourteenth and sixteenth centuries.

Pirenne believed that traces of a modern economy had actually emerged first in the cities but that popular reactions to its social and economic effects had forced it out of the cities and into the country. The resistance of the cloth workers to their gradual proletarianization had stifled nascent capitalist (or proto-industrial) forms of production in the cities; craftsmen rose up against the patrician governments of Ghent in 1275, Ypres in 1280, Bruges in 1280 and again in 1302, and Saint-Omer in 1305. For a time, these craftsmen gained control of the cities and instituted regulations that protected medieval craft production and prohibited the market competition and organization of labor that threatened to reduce them to the status of paid laborers.[6] These short-lived popular regimes also launched efforts to stamp out rural competition by force. Mobs of urban cloth workers made forays into the countryside, breaking apart the looms and burning the cloth they found there. But their efforts were unsuccessful in the end. Not even Bruges and Ghent, the most powerful cities, were able to prevent rural cloth manufacturing except in their immediate vicinities. And there, they were only partly successful.

According to Pirenne and others who subscribed to his view, the craft revolt drove modern forms of production out of the cities and, in doing so, spelled the doom of these traditional centers of textile manufacturing. After the patricians regained power, municipal governments joined forces with the craft guilds in the creation and enforcement of an economic policy hostile to capitalist, unregulated

[4] Henri Pirenne, *Histoire de Belgique*, 2d edn., 7 vols. (Brussels, 1908), 2: 418–21.

[5] Pirenne wrote that rural cloth production in the late Middle Ages and early modern period was "characteristique de l'époque moderne"; *Histoire de Belgique*, 2: 416; and he claimed that it was free and capitalistic; Henri Pirenne, *Early Democracies in the Low Countries* (1910; rpt. edn., New York, 1971), 207.

[6] Pirenne, *Histoire de Belgique*, 2: 60–67; and "Place of the Netherlands," 35–36. See also Henri Laurent, *Un grand commerce d'exportation au Moyen Age: La draperie des Pas-Bas en France et dans les pays Méditerranéens (XII^e–XV^e siècles)* (Paris, 1935), 126–27; and Van Werveke, "Industrial Growth in the Middle Ages," 237–45.

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forms of production and sought to protect the medieval craft economy. They attempted to shore up urban manufacturing by relying more and more heavily on medieval urban institutions, such as market and production privileges and guild regulations. For example, they petitioned the counts and countesses of Flanders and Artois to enforce the cities' cloth-making privileges, suppress rural counterfeits, and require that rural textile centers obtain—that is, purchase—a license to produce particular types of cloth. They also continued the raids against rural textile manufacturers.[7] In their efforts to distinguish their own cloth from that made in the countryside and small towns, city authorities used guild regulations to force producers to concentrate their energies on producing the highest quality (and thus the most expensive) cloth. This, it has long been argued, was a self-defeating strategy. Imposing ever-higher standards of quality on cloth production drove the price of city-made cloth up even further in a market that, according to Pirenne, increasingly preferred moderately to low-priced textiles, thereby making urban manufacturers less and less capable of competing against the rural upstarts. These regulations also made urban production increasingly rigid and incapable of change at a time when market conditions changed rapidly. Unable to stamp out rural production and unable to compete against it, the cities could do little more than retreat behind a wall of medieval privileges and regulations.[8]

Implicit in this model is the belief that medieval and modern economic institutions and practices were mutually exclusive. According to this view, the cities whose economies were shaped during the Middle Ages were doomed to decline with the appearance of modern, capitalistic forms of manufacture and trade.[9] On the one hand, traditional urban production was monopolistic, costly, and inflexible. On the other, rural production was competitive, inexpensive, and flexible. The one form of production is seen as distinctively medieval, the other as foreshadowing the modern. Thus the model presents a series of opposites–urban and rural, uncompetitive and competitive, regulated and unregulated, declining and expanding, medieval and modern.[10] A modified version of this model was used to explain the rise of new urban centers at this time, such as Antwerp. These

[7] Pirenne described the cities' efforts to stamp out rural production as "protectionism gone mad"; "Place of the Netherlands," 38.

[8] "They [urban artisans] trusted in nothing but privilege, and, as the situation became more critical, they made it worse by the continually more exclusive regulations by which they tried to ensure for themselves the work which was leaving them"; Pirenne, "Place of the Netherlands," 39. See also the assessment of A. B. Hibbert, who describes the later Middle Ages as a period of "the multiplication, elaboration, and rigidity of these measures [urban economic monopolies]"; Hibbert, "The Economic Policies of Towns," *Cambridge Economic History*, 10 vols. (Cambridge, 1963), 3: 224.

[9] "A fundamental consequence of later urban policies was that towns were no longer in the van of economic progress . . . Like Mesozoic reptiles they speeded their decline by the very weight of their defenses and by the entrenched specialization of their structure they lost in vigour and adaptability and response"; Hibbert, "Economic Policies of Towns," 228. While Hibbert equates medieval urban economic institutions with the dinosaurs, Pirenne relegates them to the peat bogs of the primeval forest: "This was too old, too deeply rooted in tradition and use to make its modification possible. Accordingly the medieval municipal system went on vegetating and collapsed in slow decay"; *Early Democracies*, 206.

[10] Yoshio Fujii writes of a "dichotomie Pirennienne" in "Draperie urbaine et draperie rurale dans les Pays-Bas méridionaux au bas moyen âge: Une mise au point des recherches après H. Pirenne," *Journal of Medieval History*, 16 (1990): 89.

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urban newcomers surpassed their predecessors, it is explained, because they spurned the protective privileges and regulations of the medieval towns and embraced an unrestricted and competitive market economy.[11]

GIVEN THIS MODEL OF URBAN AND RURAL MANUFACTURING in the later Middle Ages, one would predict a steady decline of urban cloth manufacturing once market-oriented textile production appeared in the countryside and small towns at the end of the thirteenth century. But, in fact, the move of manufacturing from the cities to the countryside in the late Middle Ages was incomplete and temporary. Medieval forms of manufacturing continued into the seventeenth century, and, during the fifteenth century, cloth manufacturing revived in many of the old textile centers.[12] Further research has challenged a number of Pirenne's assertions about medieval textile production and in turn has led to a reconsideration of many aspects of his model. Much of the groundwork for this reconsideration was laid by Emile Coornaert, whose work has provided the starting point for subsequent research into cloth manufacturing in northern France and the Low Countries during both the medieval and early modern periods.[13] Yoshio Fujii has provided an excellent discussion of much of this scholarship.[14] Fujii's work needs no repetition, but a few trends relevant to this article will be covered below.

In opposition to Pirenne's division of Flemish cloth manufacturing into a high medieval urban phase and a late medieval and early modern rural phase, Coornaert argued that there were a number of rural textile centers manufacturing cloth during the urban apogee of the twelfth and thirteenth centuries.

[11] "The contrast between Antwerp and Bruges is one between the past and the future, between privilege and equal rights, between protection and free trade"; Pirenne, "Place of the Netherlands," 40. See also Pirenne, *Early Democracies*, in which Bruges is identified with a medieval, regulated economy and Antwerp with a capitalist, free economy; 204–06. Pirenne links the rise of Antwerp to the emergence of rural cloth production: "Naturally, the 'new cloth trade' worked for the Antwerp market"; 208. A good summation and criticism of the use of Bruges and Antwerp as representatives of two different economic orders is Wilfrid Brulez, "Brugge en Antwerpen in de 15e en 16e eeuw: Een tegenstelling?" *Tijdschrift voor Geshiedenis* (Groningen), 83 (1970). It should be noted that Antwerp in fact enjoyed a number of economic privileges and monopolies.

[12] Herman van der Wee, "Industrial Dynamics and the Process of Urbanization and Deurbanization in the Low Countries from the Late Middle Ages to the Eighteenth Century: A Synthesis," in van der Wee, ed., *The Rise and Decline of Urban Industries in Italy and in the Low Countries: Late Middle Ages–Early Modern Times* (Leuven, 1988), 358; Marci Sortor, "Urban-Rural Interdependence in Saint-Omer's Cloth Trade during the Late Fourteenth and Early Fifteenth Centuries," annual meeting of the Medieval Academy of America, Madison, Wisconsin, 1989. John Munro points out the prolonged survival of luxury-cloth centers through over a century of low-cost competition from English manufacturing and argues that such longevity indicates their success rather than their failure; "Urban Regulation and Monopolistic Competition in the Textile Industries of the Late-Medieval Low Countries," in Erik Aerts and John Munro, eds., *Textiles of the Low Countries in European History* (Leuven, 1990), 47. He remarks elsewhere that it was surprising that *drap* manufacturing in the Low Countries had survived well into the fifteenth century; John H. Munro, "Industrial Transformations in the North-West European Textile Trades, c. 1290–c. 1340: Economic Progress or Economic Crisis?" in Bruce M. S. Campbell, ed., *Before the Black Death: Studies in the 'Crisis' of the Early Fourteenth Century* (Manchester, 1991), 137. Also, see below, n. 15.

[13] Emile Coornaert, *Une industrie urbaine du XIVe au XVIIe siècle: L'industrie de la laine à Bergues-Saint-Winoc* (Paris, 1930); Coornaert, *Un centre industriel d'autrefois: La draperie-sayetterie d'Hondschoote (XIVe–XVIIIe siècles)* (Paris, 1930); and Coornaert, "Draperies rurales, draperies urbaines: L'evolution de l'industrie flamande au moyen âge et au XVIe siècle," *Revue belge de philologie et d'histoire*, 28 (1950).

[14] Fujii, "Draperie urbaine et draperie rurale."

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Additionally, he noted that urban centers did not decline with the speed predicted by Pirenne's model and that many of the new centers emerging after the end of the fourteenth century were urban, not rural. Moreover, a number of rural centers declined during the fifteenth century, while some of the traditional cloth manufacturing cities in Flanders and northern France experienced a new phase of expansion beginning in the late fifteenth century.[15] Pursuing these issues, David Nicholas has argued that the cities of the time did not necessarily consider all rural manufacturing to be a threat. Acts of urban aggression against rural textile manufacturing were limited to efforts to suppress counterfeit versions of guild cloth; the production of other types of cloth was tolerated.[16]

Coornaert suggested that rural and urban manufacturing were not two opposed processes. Rural cloth was sold on urban markets and, in the case of the lower-priced serge industry (*sayetterie*) of Hondschoote, urban and rural producers jointly manufactured cloth. For example, the city of Saint-Omer dyed cloth woven in "rural" Hondschoote.[17] Confirming Coornaert's observations and developing them further, Nicholas has demonstrated that Bruges marketed rural textiles. In fact, the Flemish city succeeded in monopolizing the distribution of rural cloth produced within its vicinity. He also asserts that rural craftsmen worked for master craftsmen of Ypres until the last two decades of the thirteenth century. These findings have led Nicholas to speak of urban-rural relations in terms of reciprocity and equilibrium rather than in terms of opposites.[18] Similarly, Alain Derville has argued that the early fourteenth-century city of Saint-Omer served as a market for rural serges, or *says* (cloth of medium quality made from lesser grades of wools) destined for export, and he has surmised that this role as a distributor of rural cloth continued into the fifteenth century. Derville has also argued that Lille recovered as a cloth center in the fifteenth century, thanks in good measure to the sale and finishing of cloth made outside the city.[19] Denis Clauzel and Sylvain Calonne have demonstrated the considerable importance of these two functions for Lille's prosperity in the later Middle Ages.[20] Clearly, it is time, as Fujii proposes, to reconsider whether urban and rural industry were truly opposed and to look at the ways in which they were complementary.[21]

Coornaert challenged Pirenne's neat opposition of expensive urban cloth made with English wool to inexpensive rural cloth made with indigenous or Spanish wools by showing that the production regulations of some cities specified the use of local, Irish, Scottish, and other inferior wools while the regulations of certain

[15] Coornaert, "Draperies rurales," 63, 69–72, 80–81. Proto-industrialization was not an exclusively rural economic form; it was practiced in medieval and early modern cities as well as in the countryside; Paul M. Hohenberg and Lynne Hollen Lees, *The Making of Urban Europe, 1000–1950* (Cambridge, Mass., 1985), 130; van der Wee, "Industrial Dynamics," 318.

[16] Nicholas, *Town and Countryside*, 76–116, 203–21.

[17] Coornaert, *Une industrie urbaine*, 36, 43; *Un centre industriel d'autrefois*, 27; and "Draperies rurales," 76.

[18] Nicholas, *Town and Countryside*, 85–87, 95–97, 105–06, 203.

[19] Derville, "Draperies flamandes," 370; Alain Derville, "L'héritage des draperies médiévales," *Revue du Nord*, 69 (1987): 721.

[20] Denis Clauzel and Sylvain Calonne, "Artisanat rural et marché urbain: La draperie à Lille et dans ses compagnes à la fin du Moyen Ages," *Revue du Nord*, 72 (1990): 540, 550–51, 557.

[21] Fujii, "Draperie urbaine et draperie rurale," 89.

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rural centers required that cloth workers use only the finest English wool.[22] Developing this position, Derville questions whether English wool imports could possibly have been sufficient to supply the manufacturers of all the cloth towns of Flanders and northern France.[23] Coornaert also argued that, far from being limited to the production of expensive luxury textiles, these cities produced a variety of types of cloth.[24] More recently, Derville has questioned whether the apex of the urban *grandes draperies* occurred in the thirteenth century at all.[25] The findings of Patrick Chorley and Hilmar Kreuger support both Coornaert's and Derville's reconsideration of this aspect of Pirenne's theory. Chorley and Kreuger have demonstrated that the great cloth manufacturing cities of the twelfth and thirteenth centuries in fact exported far more moderately priced textiles such as *says* (serges), *stanforts* (moderately to inexpensively priced textiles of medium quality), *biffes* (very light, inexpensive cloth), and the less expensive varieties of *drap* (fine woolen) such as *rays* (striped cloth) than they did the highest quality *drap*. The records of Genoese merchants from the second half of the twelfth and the early thirteenth centuries indicate that *says* from French and Flemish cities were the most popular exports.[26] And several of the traditional cloth centers, including Arras, Saint-Omer, and Bruges, exported primarily or exclusively moderately priced and inexpensive textiles to Iberian, Italian, and southern French markets during the second half of the thirteenth century. Even those cities renowned for their luxury textiles, such as Ypres, Ghent, and Provins, derived much of their prosperity from the manufacture and export of *biffes*, *rays*, and *stanforts*.[27] The cities' heavy reliance on the manufacture of luxury cloth was a phenomenon of the late thirteenth and fourteenth centuries, not the twelfth and thirteenth.[28]

Thus the cities' concentration on the production of expensive cloth in the late thirteenth and the fourteenth centuries was not proof of their inflexibility or the inevitable result of a sort of economic determinism; it was a choice that manufacturers made, presumably for good reason. According to John Munro, the strategy whereby cities concentrated on manufacturing luxury woolens and imposed ever higher and more costly production standards, once seen as a perverse reaction to market conditions that favored inexpensive cloth, should be considered a rational response to economic and political changes. Given the relatively high cost of urban labor, rising transactions costs resulting from warfare and piracy, and increasing taxes, the urban cloth makers' concentration on the manufacture of expensive textiles for export in the early fourteenth century was

[22] Coornaert, "Draperies rurales," 64–65. See also A. E. Verhulst, "La laine indigène dans les anciens Pays-Bas entre le XII[e] et le XVIII[e] siècle," *Revue historique*, 96 (1972): 283–301.

[23] Derville, "Draperies flamandes," 356–57; see also Patrick Chorley, "The Cloth Exports of Flanders and Northern France during the Thirteenth Century: A Luxury Trade?" *Economic History Review*, ser. 2, 40 (1987): 373–74.

[24] Coornaert, "Draperies rurales," 81.

[25] Thirteenth-century Saint-Omer exported far more moderately priced *says* than it did fine woolen cloth; Derville, "Draperies flamandes," 364–67.

[26] Hilmar Kreuger, "The Genoese Exportation of Northern Cloth to Mediterranean Ports, Twelfth Century," *Revue belge de philologie et d'histoire*, 65 (1987): 744.

[27] Chorley, "Cloth Exports of Flanders and Northern France," 363–67.

[28] Munro, "Industrial Transformations," 114–15.

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a sensible strategy. The high cost of the industry's most important raw material—since the fine English wool used in these textiles accounted for 65 to 75 percent of the pre-finishing manufacturing costs—made further measures to increase the quality of their product equally sensible.[29] Moreover, Munro argues that the efforts of each luxury-cloth center to distinguish its products from those of the other producers made good sense in market terms. The more that buyers preferred a particular type of luxury cloth, the more they would be willing to pay for it. If the manufacturers of a given textile center were successful in capturing consumer demand, the increased costs of production would pay off.[30] The success of this strategy is illustrated by the fact that cloth manufacturers in many villages and small towns eventually followed the traditional urban centers' lead by improving the quality of their woolens. By the mid-fourteenth century, they produced cloth that, though cheaper than the expensive urban textiles, was not inexpensive.[31] Far from being the commercial dinosaurs described in Pirenne's model, then, the traditional textile cities were path breakers, and the rural producers followed their lead.

Coornaert also questioned Pirenne's assumption that rural cloth production was necessarily free of the regulations that grew increasingly numerous and restrictive in the fourteenth century. According to Coornaert, new textile centers—rural as well as urban—sought comital recognition of their right to produce particular types of cloth and, in doing so, established production specifications and quality regulations just as the traditional urban centers did.[32] Subsequently, other aspects of the growth of production regulations in the early fourteenth century have been explored. Derville has argued that the motivating force behind production regulations was the merchants, not artisans opposed to market competition. Regulations created a level of standardization of products that facilitated long-distance trading in bulk.[33] And, according to Munro, the shift to luxury-cloth production necessitated increasingly strict quality regulations. Regulations also served to distinguish one city's luxury woolens from the others.[34] Thus production regulations are not necessarily evidence of the cities' inherent

[29] Munro, "Industrial Transformations," 120–27; and "Urban Regulations," 43. The consequent heavy reliance on fine English wools soon created overwhelming difficulties for the cities. Regulations on quality, such as the prohibition of machine fulling (felting), did increase labor costs, but Munro argues that the impact of these increases was "proportionately minimal, and swamped by rising English wool prices"; "Urban Regulations," 46.

[30] Munro, "Urban Regulations," 44. Oddly, this explanation continues to convey a sense of inevitability in regard to the decline of traditional centers of cloth production: they were trapped in a set of market forces that forced their manufacturing to become increasingly inflexible and expensive and that separated them from the largest portion of the market.

[31] Munro, "Industrial Transformations," 114–15, 138.

[32] Coornaert, *Un centre industriel d'autrefois*, 151; "Draperies rurales," 84–85. Van Werveke provides a list of small towns and villages possessing cloth production privileges in the fourteenth century; "The Low Countries," in *Cambridge Economic History*, 3: 355. See also Georges Espinas, *La draperie dans la Flandre*, 2 vols. (Paris, 1923), 1: 55. Espinas attempted to resolve the apparent conflict between Pirenne's theory and his findings by identifying as urban all locations that used seals.

[33] Derville, "L'héritage des draperies médiévales," 721.

[34] Munro, "Industrial Transformation," 138. For the sixteenth century, van der Wee has argued that guilds and their enforcement of production regulations could assist in the transformation of a city's economy, particularly in its shift in production to high-quality and high-priced goods such as luxury textiles, tapestries, and to jewelry making and the arts; "Structural Changes," 212–13.

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inability to compete; they should be considered in terms of the advantages they offered the cities—and villages—that adopted them. Curiously, regulations and other associated economic institutions like guilds are still considered, even by some of those who have illustrated their benefits, to be one of the distinctive attributes of medieval urban textile centers that contributed to their ultimate decline.[35]

DESPITE THE CHALLENGES POSED BY THE FACTS, Pirenne's opposition of medieval urban production and modern rural manufacturing has survived for nearly a century, outlasting the laissez-faire economic theory on which it was based. Historians continue to rely on the concept of an urban-rural dichotomy to explain industrialization and its setbacks in the medieval and early modern periods, because it has become part of the way we think about the differences between the modern and medieval economies and what we believe to be the definitive characteristics of each. For example, a recent work on the development of the modern economy in the Low Countries explains away the failure of rural production to prevail against urban manufacturing in fourteenth and fifteenth-century Flanders by arguing that warfare at the end of the fifteenth and the beginning of the sixteenth century drove textile manufacturing back within the protective walls of the cities and back into a preindustrial mode of production. Industrialization then had to wait until the seventeenth century to make its definitive appearance in the Flemish countryside.[36] Without question, during times of war, rural population and wealth flowed to the cities. But this argument automatically rejects the possibility that these cities possessed advantages in manufacturing other than their protective fortifications. It fails to consider the implications of the fact that medieval cloth-manufacturing cities continued to survive and even prosper long after the appearance of market-oriented cloth production in the countryside and small towns throughout the Low Countries.

Only recently has the established dichotomy been questioned, and then, only in part. In their study of European urbanization, Paul M. Hohenberg and Lynne Hollen Lees criticize the periodization of urban development into just two phases: medieval and modern. They propose an intermediary proto-industrial phase.[37] Similarly, Martha Howell rejects the bipolar model with its assumption that capitalism and medieval craft production were the only possible productive forms when she argues that a third economic form, small commodity production,

[35] "Corporatism" is generally considered a problem facing late medieval and early modern cities. See van der Wee, "Structural Changes," 208; "Industrial Dynamics," 324. John Munro has contrasted the "freedom of enterprise" in England to the Flemish towns with their guilds; "Bruges and the Abortive Wool Staple in English Cloth: An Incident in the Shift of Commerce from Bruges to Antwerp in the Late Fifteenth Century," *Revue belge de philologie et d'histoire*, 44 (1966): 1139. Clauzel and Calonne categorize those villages with elements of corporatism—guilds and regulations—as "proto-urban" and argue that cloth-producing villages without such institutions were free of major social conflicts; "Artisanat rural et marché urbain," 564–65. See also Coornaert, *Une industrie urbaine*, 44–45.

[36] Gutmann, *Toward the Modern Economy*, 43–44, 48–49.

[37] Hohenberg and Lees, *Making of Urban Europe*, 101–05. See also Jan De Vries, *European Urbanization, 1500–1800* (Cambridge, Mass., 1984), 4–10.

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existed in some fifteenth-century German towns. Unlike the proto-industrial phase proposed by Hohenberg and Lees, Howell's model of small commodity production represents a distinctive economic form rather than a transitional stage between medieval and modern production.[38] Despite the changes they propose, none of these authors abandons the old model completely. Hohenberg and Lees believe that the division of the urban economy into medieval and modern stages does not represent economic developments accurately, but they introduce a third stage without reconsidering traditional assumptions about the medieval economy and its institutions. Howell also rejects the old model in her explanation of the economic developments of cities that thrived in the early modern period, such as Leiden, but then resurrects the paradigm of opposites in an effort to distinguish small commodity production from medieval craft production. She describes medieval craft production as "a stagnant holdover from earlier days found in economic sectors which were in decline," a "backwater . . . artificially preserved," and contrasts it with modern capitalist production, which she discovers in "growing, market-oriented industries."[39] In effect, medieval forms of production and the cities that grew strong using them were bound to decline; and, to the extent that they did not decline as rapidly or as completely as expected, they were sustained by measures that went against the natural forces of the market economy. Such an explanation dismisses developments taking place in the traditional medieval urban centers during this period and sheds little light on their continuing importance in the regional and international economies. Thus the old model remains, augmented but essentially intact; medieval cities and their institutions are still destined to decline in the face of new economic forms in the fourteenth and fifteenth centuries.[40]

Herman van der Wee has challenged another assumption central to the urban-rural model—that the two were mutually exclusive—and its corollary that the medieval craft production of the cities could not survive simultaneously with more "modern" forms of rural production. By considering long-term trends in both urban and rural production, and by including all forms of manufacturing in his analysis, van der Wee provides considerable insight into the forces acting on old and new textile centers. He proposes that the possibility of industrial "life-cycles," the mobility of merchant capital, and the nature of commercial growth in an era when technological change offered little chance for increased profits, may explain the shift of textile manufacturing to Brabant and Holland

[38] Martha C. Howell, *Women, Production, and Patriarchy in Late Medieval Cities* (Chicago, 1986), 33–38, 64–69. Howell defines small commodity production as a form of manufacturing for export that mediated between the competitiveness of the capitalist, market-oriented economy and the traditional urban values of craft production and thus served as an alternative. See also Robert DuPlessis and Martha Howell, "Reconsidering the Early Modern Economy: The Cases of Leiden and Lille," *Past and Present*, 94 (1982): 84.

[39] Howell, *Women, Production, and Patriarchy*, 38, 68. Like Howell, Hohenberg and Lees distinguish "busy centers" from the "holdovers from the days of the medieval crafts"; *Making of Urban Europe*, 126.

[40] Derville points out that the argument for the existence of a third, non-medieval, non-capitalist phase is based on the assumption that capitalism and economic regulations were mutually exclusive. He argues that commercial capitalism and regulation were in fact compatible; "L'héritage des draperies médiévales," 723.

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and the gradual decline of the older urban cloth centers in both Flanders and northern France.[41] Taking into account long-term economic developments and demographic shifts in both the cities and countryside of the Low Countries, van der Wee has demonstrated that changes in the distribution of wealth, and thus in market demand for different types of cloth, sometimes favored urban manufacturing over rural and sometimes benefited both forms of cloth production.[42] Rather than competing directly against each other, rural and urban manufacturing were two separate, indeed "autonomous," processes oriented toward different markets and thus affected by different market forces.[43] However, the assertion that rural and urban manufacturing were not in competition with each other in the late medieval and early modern periods because they were completely separate processes, oriented toward completely different markets and operating according to separate principles, maintains and even strengthens the idea of an urban-rural dichotomy despite evidence that, at least in some cases—such as the organization of labor, the type of raw materials used, the type of cloth produced, and the possession of production privileges—rural manufacturing did not necessarily differ significantly from its urban counterpart.

UNQUESTIONABLY, DIFFERENT CONDITIONS existed in the cities and the countryside. In particular, wages were lower in the country than in the city, and this attracted profit-minded merchants, especially those dealing in inexpensive textiles, to rural locations. But the extent to which these differences indicate that urban medieval economic institutions made the traditional cloth-making cities uncompetitive and unresponsive to market changes, or that urban and rural manufacturing were completely separate processes (whether mutually exclusive or autonomous), remains unknown. The remainder of this article will explore the relationship between medieval cities and the countryside by examining the ways in which one traditional urban textile center, Saint-Omer, responded to the presence of rural manufacturing in the later Middle Ages. As the following discussion of municipal regulations concerning the manufacture, distribution, and consumption of cloth will show, the city's position in manufacture and trade was plagued by neither rigidity nor inflexibility. Saint-Omer developed a range of strategies for responding to competition and changes in market demand, some that incorporated medieval economic institutions such as regulations and privileges and others that minimized the role of those institutions. Moreover, the economic policies of Saint-Omer's government and the actions of its merchants and craftsmen indicate that rural and urban manufacturing did not belong to separate or mutually opposed economic systems, the one medieval and the other modern. In the case of Saint-Omer, urban and rural manufacturing were sometimes complementary and interdependent.

Saint-Omer, now included in the French department of Pas-de-Calais, was

[41] Van der Wee, "Structural Changes," 204–06; "Industrial Dynamics," 323–44.
[42] Van der Wee, "Industrial Dynamics," 317–18.
[43] Van der Wee, "Industrial Dynamics," 372.

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located slightly south of the fourteenth-century border between Flanders and the French county of Artois.[44] Like the other great cities of thirteenth-century Flanders and France, Saint-Omer had flourished on the manufacture and export of cloth, particularly *says*. By 1250, the city's cloth workers produced some 60,000 *says* annually.[45] In the early fourteenth century, Saint-Omer's population consisted of at least 35,000 inhabitants, making it one of the largest cities in France and the Low Countries.[46] But, by the turn of the century, the early prosperity seemed to vanish. Saint-Omer's *sayetterie* experienced a dramatic decline between 1299 and 1310. During that time, the city had difficulty finding anyone willing to purchase the farm for weighing *says*, and even after a mild recovery in 1315–1320, the farm was less than 7 percent of its late thirteenth-century level.[47] While the French city's *sayetterie* languished, the rural production of *says* apparently continued undiminished, and Bruges' craftsmen began to produce counterfeit versions of Saint-Omer's *says*.[48] Popular revolt overthrew the patrician government in 1305; soon afterward, the countess of Artois supported a successful patrician effort to regain power. Clearly, the city was in a crisis. But these difficulties should not be taken as proof that Saint-Omer faced inevitable decline because its economy was medieval rather than modern. Like the other traditional urban cloth centers in Flanders and northern France, its manufacturers began to produce increasing amounts of the more expensive and luxurious *draps* in the fourteenth century. In particular, Saint-Omer's cloth workers concentrated their energies on the production of *rays*. This striped cloth, though classified as a *drap*, was intermediate in price and quality between the expensive traditional *drap* and the inexpensive *say*.[49]

Whether they manufactured *rays*, *says*, or *draps*, Saint-Omer's cloth workers and merchants were willing to modify cloth production to meet market demand and to remain competitive with other textile centers. The willingness of producers to change production regulations in order to suit new or changing markets can be seen in the early fourteenth century when, in response to English preferences, city leaders allowed weavers making cloth for England's markets to weave *rays* larger than the regulations specified.[50] Later that century, falling demand for Saint-Omer's *draps* prompted a change in production regulations.[51] And, from time to time in the fifteenth century, the municipal government subsidized trips to other textile centers in order to compare and improve manufacturing tech-

[44] For Saint-Omer's history, see Alain Derville, ed., *Histoire de Saint-Omer* (Lille, 1981); A. Derville, *Saint-Omer des origines au début du XIV^e siècle: Essai d'histoire sociale* (unpublished thesis, Sorbonne, 1971); Georges Espinas, *Les origines du capitalisme*, III: *Deux fondations de villes dans l'Artois et la Flandre française (X^e–XV^e siècles); Saint-Omer et Lannoy du Nord* (Lille, 1946); Arthur Giry, *Histoire de la ville de Saint-Omer et de ses institutions jusqu'au XIV^e siècle* (1877; rpt. edn., Geneva, 1977).

[45] Derville, "Draperies flamandes," 366–67.

[46] Alain Derville, "Le nombre d'habitants des villes de l'Artois et de la Flandre Wallonne," *Revue du Nord*, 65 (1983): 277–300.

[47] Nine livres compared to 131 l. 6 s. 8 d.; Derville, "Draperies flamandes," 366–67.

[48] Derville, "Draperies flamandes," 367, 369.

[49] Derville, "Draperies flamandes," 365–68.

[50] See *ca.* 1306, Saint-Omer, Registre aux Bans Municipaux, as transcribed by Georges Espinas and Henri Pirenne, in *Recueil de documents relatifs à l'histoire drapière en Flandre: Première partie, Des origines à l'époque bourguignon*, 4 vols. (Brussels, 1906–20), 3: 280–81.

[51] See 1350–75, Saint-Omer, Bans Municipaux, in Espinas and Pirenne, *Recueil*, 328–31.

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niques.[52] Additionally, in the late fourteenth and fifteenth centuries, Saint-Omer's merchants, cloth workers, and government modified production to meet the specific demands of eastern and Baltic markets—particularly those in Russia—and of the Hanseatic merchants who provided access to them.[53] The city government also worked to forge close commercial ties with the Hanse by making special provisions for German merchants and by granting them privileged status. In 1389, the city prohibited its residents from driving up the price of cloth through speculative buying upon the arrival of German merchants.[54] That same year, the municipal government established a special panel to hear disputes involving its citizens and members of the Hanse.[55] Throughout the fifteenth century, Saint-Omer's government sent representatives to the Hanse headquarters at Bruges to negotiate textile production standards, such as the kind of wool, the fineness of the yarn, and the quality of the fulling (a process of shrinking and thickening that felts the cloth).[56] The city even appointed an inspector whose job it was to ensure that its cloth met Hanse specifications.[57] Clearly, Saint-Omer's merchants and craftsmen were responsive to changes in market demand, and the urban environment did not necessarily preclude flexibility in production. Its success in becoming a supplier of cloth to the Hanse, and thus in producing for the increasingly important markets in northern and eastern Europe, cannot be explained away as a kind of default based on the depredations of war in the countryside; the Hanse purchased similar types of cloth from rural producers at this time as well. In fact, during the second half of the fifteenth century, the Flemish village of Poperinghe was one of the French city's primary competitors for the German merchants' business.[58]

It has been argued that efforts to improve or correct production quality are

[52] In 1416, delegates were sent to "several cities in Flanders and Artois" to investigate dyeing techniques; 1416–17, Saint-Omer, Archives Municipales, Comptes des Argentiers. In January of 1460, a delegation was sent to the "principal cities and places of Flanders." On two other occasions that year, groups were sent to Bruges, Ypres, Courtrai, Menin, and Wervicq to inquire into *drap* weaving and dyeing; 1459–60, Registre aux Délibérations, f° xliii v°; 1460, Comptes des Agentiers, f° 68 r°, as transcribed in Justin de Pas, "Documents sur l'industrie drapière à Saint-Omer," *Memoires de la Société des Antiquaires de la Morini*, 31 (1913): 64–69. These last two trips apparently were part of an effort to redirect production toward expensive types of cloth, "draps fines."

[53] See 1459, Registre aux Délibérations, f° xlii r°; 1471, Comptes des Argentiers, f° 71 r°, in de Pas, "Documents sur l'industrie drapière," 65–66, 71–72.

[54] See 1389, as transcribed in Karl Kunze, ed., *Hansisches Urkundenbuch*, vol. 4: *Mit einem Sachregister*, 7 vols. (Halle, 1896), 4: 435. Records also make mention of the *Osterlins*; 1482–83, Comptes des Argentiers, f° 83 v°; and Registre aux Délibérations B *bis* f° 70 v°–71 r°. De Pas also cites a 1433 reference to *Osterlins*; "Documents sur l'industrie drapière," 72.

[55] See 1389, Saint-Omer, Archives Municipales, BB. 181.18.

[56] See 1416–17, 1417–18, 1432–33, 1447–48, 1458–59, 1464–65, 1470–71, 1482–83, Saint-Omer, Archives Municipales, Comptes des Argentiers. Also, 1452, Registre aux Délibérations, B f° xx r°; and 1459, f° xlii r°, in de Pas, "Documents sur l'industrie drapière," 61, 65–66. Pagart d'Hermansaart claimed that the Hanse stopped visiting Saint-Omer's market and stopped purchasing its cloth in 1460, when the city ceased using Spanish wools; "Les anciennes communautés d'arts et métiers à Saint-Omer," *Mémoires de la Société des Antiquaires de la Morini*, 16 (1879–81): 542. The documentary evidence does not support his assertion.

[57] See 1464–65, Saint-Omer, Archives Municipales, Comptes des Argentiers. See also 1465–66, Comptes des Argentiers, in de Pas, "Documents sur l'industrie drapière," 70–71. This office was discontinued in 1496; 1495–96, Comptes des Argentiers, f° 121 v°, in de Pas, "Documents sur l'industrie drapière," 73.

[58] See 1470–71, Comptes des Argentiers, f° 71 r°, in de Pas, "Documents sur l'industrie drapière," 71–72.

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proof of the deterioration of Saint-Omer's cloth industry. The evidence cited in arguments of this sort covers a period of nearly two hundred years, however, making the supposed deterioration excruciatingly protracted, to say the least.[59] While there is no doubt that most of the improvements were responses to problems facing its cloth industry (Saint-Omer's manufacturers rarely if ever modified production regulations in anticipation of market changes), to interpret them as evidence of a single process of decline explains very little. It seems more profitable to consider such regulations as part of a process of modification in response to demand.[60] Such a process was not without its difficulties and, at times, its failures. Nevertheless, changes in textile production regulations should be seen as efforts to produce what consumers or commercial intermediaries like the German merchants wanted. As such, these changes reveal the forces acting on urban producers and, to the extent that they made similar textiles for the same market, on rural producers as well. For example, during the second half of the fifteenth century, Hanse merchants demanded that Saint-Omer provide good quality cloth at low prices. They kept a steady downward pressure on prices by pitting the city's cloth workers against rural producers, all the while insisting that quality levels not sink accordingly. Although one could characterize the Hanse strategy as forcing cities to play a game that they could not win, the continued participation of the urban centers and the development of new ones throughout this period suggest an alternative interpretation. The existence of this tension between quality and price helps explain why cities could and did compete with rural centers. Quality *was* important, and cities could better regulate and guarantee quality. Moreover, in the cases cited above, privileges, production regulations, and inspections—those alleged villains of rigidity and uncompetitiveness—played a central part in the city's adaptation to new market demand. Saint-Omer's medieval economic institutions provided a framework within which the city could respond to changes in demand and continue to compete against other producers.

Not all aspects of Saint-Omer's response to changes in market demand and the appearance of new competition relied on traditional institutions. From the second half of the fourteenth century, the municipal government consciously sought to support and protect the production of inexpensive types of cloth within city walls by exempting them from many of the costs that are generally assumed to have been inescapable in an urban setting. In a strategy that goes against longstanding assumptions about urban manufacturing, Saint-Omer's government recreated some of the conditions commonly associated with production of cloth in the countryside, not the city. Provisions specified that certain types of cloth would not be subjected to the production requirements and inspections traditionally imposed on that produced within the city: "Every foreign cloth producer [*drappier*] may reside in the city [of Saint-Omer] and make all the *demi-draps* that he wishes, whether he uses lambswool or other types of wool, without [being subject to]

[59] The evidence ranges from the end of the thirteenth century to the end of the fifteenth; de Pas, "Documents sur l'industrie drapière," 53–58; and Derville, "Draperies flamandes," 366–67.

[60] Additionally, these periodic crises and revisions should be considered in light of phases of expansion and contraction in the urban textile industries.

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regulations or [requiring] the city seal, upon paying 4 deniers per livre on the value of the above-said *demi-draps*, and all masters in the cloth trades may work on these as much as they wish, each day, without [being subject to] inspection."[61] While *demi-draps* could be expensive, the freedom to use any sort of wool (in particular, lambswool was inferior to the wool of adult sheep) and the absence of inspections indicate that at least some of the textiles covered by this regulation were inexpensive. Not only did Saint-Omer's leaders seek to develop the production of inexpensive cloth, they also attempted to draw on outside labor and resources in the process. These provisions specifically addressed masters and merchants who were not residents of Saint-Omer. Foreign manufacturers were not required to join the city's bourgeoisie or its cloth guilds or to pay any entry fees, and the cloth they produced was free of the costs and restrictions imposed by inspections. They did pay a fee based on the value of the cloth, 4 deniers per livre, which was well below the cost of the measurements and inspections required of the best and medium-quality cloth.[62] These liberal provisions kept down the costs of production within the city and allowed manufacturers the flexibility needed to meet changes in market demand by freely modifying their product. In this way, Saint-Omer cut the cost of urban production where it hurt the most—inexpensive textiles—and drew on outside enterprise to do so.[63]

Flexibility in cloth production and cost-cutting measures were part of Saint-Omer's answer to the challenge posed by changes in markets and the growth of manufacturing in villages and small towns. But its response to changes in the late medieval economy cannot be explained simply in antagonistic terms, such as its ability to compete against other producers, particularly the rural textile producers. Part of the city's ability to adapt to the rise of cloth making in small towns and

[61] "Item, tout drappier estraigne pooroient venir demourer en le ville [de Saint-Omer] et faire tout desmi-draps retrais tels que il leur plairoit, soit d'angelins ou d'autres laines, sans ley [loi] ne sans seng [sceau] de le ville, parmi paiant 4 deniers de le livre de valeur des dessussdits demi-draps et sur ce ouvrer de tous mestiers de le draperie tant que il leur plairoit cascun jour, sans astraint ne esgart [esward]"; see *ca.* 1350–75, Saint-Omer, BB. 134.4, in Espinas and Pirenne, *Recueil*, 328–31. Espinas discussed this document in terms of the fees levied on foreign drapers in *La draperie dans la Flandre*, 2: 786. He argued that this legislation allowed foreign drapers to hire craftsmen in the city to manufacture cloth. The term *drapier*, however, was used in Saint-Omer's documents to refer to individuals who themselves manufactured cloth, for the document states that they "can . . . make," rather than "can have made," the cloth in question. Coornaert distinguished the *drapiers* of sixteenth-century Hondschoote, who were cloth makers who wove *says* or prepared the warp for weavers, as well as entrepreneurs who organized all stages of the manufacture of the cloth and owned the product throughout the entire manufacturing and finishing process, from the "ancien drapier flammand qui 'facit faire draps'" and the English draper; Coornaert, *Un centre industriel d'autrefois*, 272–73.

[62] This fee is identical to the standard sales tax levied in Saint-Omer; 1320–79, Saint-Omer, Archives Municipales, BB. 1.2-.29. Cloth that was manufactured according to guild specifications had to be inspected after each stage. Additionally, it had to be weighed and measured after the weaving and fulling stages. In the late thirteenth century, the price of one aspect of inspection—measuring the cloth—varied from 4 to 16 deniers, depending on the size of the cloth and the location where the service was performed; 1270 and 1281, Saint-Omer, Bans Municipaux, in Espinas and Pirenne, *Recueil*, 238, 259. See also below, n. 63.

[63] This document identified three categories of cloth made in the city: the best-quality cloth, which was the largest at 15 *quartiers* and the most expensive, a medium-quality cloth 13 *quartiers* in width made from medium-grade wool, and those woolens made of the "third wool," both *draps* and *demi-draps* of 11 *quartiers*, for which the city required no inspections or measurements before sale; see *ca.* 1350–75, Saint-Omer, Archives Municipales, BB. 134.4, in Espinas and Pirenne, *Recueil*, 328–29.

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villages derived from the willingness of its merchants and craftsmen to draw on resources in the countryside and, in turn, to provide services to rural textile manufacturers. It is true that, in the production of certain types of cloth, urban and rural manufacturers were competitors, and relations between them could be hostile. Of course, the same could be said about relations among the cloth-producing cities at this time.[64] But the case of Saint-Omer indicates that rural and urban textile manufacturing were interdependent. This interdependence throws into question the assumptions that the modern economy was born in the countryside while the medieval economy wasted away behind the walls of the old textile cities and that rural and urban manufacturing were completely separate forms of production.

The very nature of the late medieval market economy encouraged interdependence. The production of textiles at any location—rural as well as urban—tended to go through rapid boom and bust cycles. A glance at the tax revenues derived from the sale of cloth or the revenues derived from the purchase of seals at a number of textile centers reveals wide and rapid fluctuations in production. For example, during the fourteenth and fifteenth centuries, tax revenues from the cloth industries of Langemark, Wervicq, and Courtrai varied substantially from year to year, and dramatic changes in production levels continued into the sixteenth century, as can be seen in the case of Hondschoote.[65] Given the high level of division of labor in medieval cloth manufacturing, the fluctuation in demand for labor in a city undergoing a boom, or the subsequent bust, could be considerable. Most references to cloth producers are to weavers, but these comprised only a fraction of the cloth-working population. There were the skilled crafts of dyeing, tentering (stretching), fulling, teaseling (napping), and shearing the cloth.[66] In addition, a substantial number of people were needed to process the wool before it ever reached the weavers' looms: breaking the wool apart, sorting the pieces according to their quality, cleaning (there were a number of stages of removing foreign matter from the wool), washing, greasing (if it was to be used to make the more expensive sorts of cloth), as well as carding or combing, spinning, and winding the thread on bobbins. Preparing the wool and spinning it into yarn or thread, processes performed almost exclusively by women, were very labor intensive, far more intensive than the weaving of the cloth itself. It took up

[64] The relationship among cloth-producing cities in this region possessed both competitive and cooperative aspects. Members of the Hanse des XVII Villes shared information on production and market regulations; F. Vercauteren, "Note sur la survivance de la Hanse des XVII Villes du XV^{e} au XVII^{e} siècle," *Revue belge de philologie et d'histoire*, 28 (1950): 1079–91; Henri Laurent, "Nouvelles recherches sur la Hanse des XVII Villes," *Moyen âge*, 3d ser., 6 (1935): 81–94. Like the newer centers in the countryside and small towns, however, these cities were also willing to counterfeit each other's cloth. Bruges' manufacturers counterfeited Saint-Omer's cloth; Derville, "Draperies flamandes," 365. Similarly, the cloth workers at Arras agreed to make cloth like Saint-Omer's for the Hanse. In turn, Saint-Omer produced imitations of cloth woven at Arras, Tournai, and Valenciennes; Coornaert, "Draperies rurales," 71, 81.

[65] Coornaert, *Un centre industriel d'autrefois*, appendixes V and V *bis*. See also Pierre Deyon and A. Lottin, "Evolution de la production de la production textile," *Revue du Nord*, 49 (1967): 29–33.

[66] After the cloth had been fulled, tentered, and dried, craftsmen raised the nap of the cloth by teasling it. Shearers then cut the nap to a short, even length. Van der Wee refers to thirty-two different processes in the manufacture of cloth; "Structural Changes," 204. Not all types of cloth went through every stage—for example, *says* were lightly fulled, most worsteds were not fulled, and some cloth was sold unfinished.

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to twenty women to supply a weaver and his assistant with yarn.[67] Lastly, there were a host of lesser folk referred to as "menu peuple" in the municipal records, who were associated either directly or indirectly with the manufacture and trade in cloth.

During the fifteenth century, Saint-Omer's boom and bust cycles were linked to the tendency of the German Hanse to patronize one city for a short time and then to favor another. By the mid-fifteenth century, the Hanse began to arrange with textile manufacturing centers to purchase their entire output.[68] Although this could be very profitable for the centers so favored, it also exposed them to considerable risk. The Hanse could easily transfer its patronage from one center to another, which it did if it found lower prices elsewhere or if the merchants and the producers disagreed over the conditions of sale or the quality of the product. For example, in 1459, the Hanse imposed its requirements on Saint-Omer concerning the quality of cloth, its delivery, credit extended to the merchants, and the type of currency the German merchants would use to pay their debts.[69] Faced with the possibility of losing Hanse patronage, Saint-Omer's officials could do little more than protest and then comply. Cities could not maintain a population of cloth workers large enough to take full advantage of the booms without having to face high levels of unemployment during the periods of bust. In addition to overloading the city's charitable institutions and imposing hardship on its cloth workers, severe unemployment or underemployment could lead to popular revolt and thus posed a political threat to patrician governments.[70] If Saint-Omer were to profit from the Hanse's new trading patterns, it needed a labor supply that could expand and contract in response to sudden fluctuations in demand.

The problems created by a sudden increase in demand for cloth are illustrated by an incident from the 1450s, when urban revolt in Flanders led Hanse merchants to boycott Flemish cloth. They turned to Saint-Omer to provide them with the cloth usually supplied by Flemish textile centers, including the great cloth-producing city of Ghent.[71] In an effort to meet the abrupt surge in demand,

[67] This figure pertains to textile production in the fifteenth and sixteenth centuries. Merry Wiesner, "Spinsters and Seamstresses: Women in Cloth and Clothing Production," in Margaret W. Ferguson, Maureen Quilligan, and Nancy J. Vickers, eds., *Rewriting the Renaissance: The Discourses of Sexual Difference in Early Modern Europe* (Chicago, 1986), 191–94. Walter Endrei calculates that weaving accounted for only 13 percent of the time involved in manufacturing a piece of cloth, while spinning, carding, and combing took up 60 percent; "Manufacturing a Piece of Woollen Cloth in Medieval Flanders: How Many Work Hours?" in Aerts and Munro, *Textiles of the Low Countries*, 14–21. The preparatory stages of breaking apart, sorting, and cleaning the wool were not included in his calculations.

[68] Philippe Dollinger, *La Hanse (XII^e–XVII^e siècles)* (Paris, 1964), 249.

[69] See 1459, Saint-Omer, Délibérations, B. f° xlii r°, in de Pas, "Documents sur l'industrie drapière," 65–66.

[70] Howell and DuPlessis argue that Lille's leaders believed that ensuring the well-being of the guild masters would maintain political stability and patrician rule; "Reconsidering the Early Modern Economy," 76–78.

[71] Documents mention "la rébellion Gantois"; 1452, Saint-Omer, Délibérations, B. f° xx r°; and "l'en n'a peu drappé en Flandre obstant la guerre contre les Gantois et aussi que les dits Alemans n'ont voulu achetter les draps de Flandres"; 1453–54, B. f° xxi v°, in de Pas, "Documents sur l'industrie drapière," 62.

During the 1450s and 1460s, cloth produced for the Hanse was referred to simply as *draps* made in "the German style." First mention of *draps* made "sur l'Alemant" is found in 1452, Saint-Omer, Registre aux Délibérations, B. f° xx r°, in de Pas, "Documents sur l'industrie drapière," 61.

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Saint-Omer's government juggled manufacturing and marketing regulations in order to maximize productivity. What becomes evident is that the municipal leaders exercised considerable flexibility in their efforts both to protect the city's long-term interests and to exploit to the fullest the opportunities that circumstances created. To do so, they relied on rural textile production and foreign cloth workers to expand temporarily the city's work force.

Like other medieval cities, Saint-Omer usually reserved its domestic market for its own textile producers by banning from its retail market inexpensive village-made cloth, which was produced for local consumption.[72] Because domestic consumers were the most reliable, though not the most lucrative market, the reservation of the retail market in moderately priced and cheap cloth for Saint-Omer's own producers protected the city's cloth workers from the vagaries of the export market economy. The requirement that all members of the cloth guilds wear only cloth made in the city served the same purpose.[73] However, the 1450s boom revealed, that, at least for Saint-Omer, all regulations and restrictions were negotiable. During the first year of the boom, the cloth guilds attempted to supply both the Hanse and the domestic market. The guilds agreed to weave sufficient cloth priced at 12 *sols* or less per *aune* (about 47 inches) for local consumers,[74] but the domestic market was ill-served by this arrangement. City leaders noted that the weavers were producing little or no cloth for the domestic market. By the end of the year, the guilds had to give up their pretensions of supplying both markets. They chose to focus on supplying the Hanse, which was

Unfortunately, the production regulations do not describe any aspects of their manufacture in detail. Evidently, these were not a single type of cloth but could be either heavy or light *draps* of middle to good quality; 1459, Registre aux Délibérations, f° xlii r°, in de Pas, "Documents sur l'industrie drapière," 65–66. Records of disputes between the city and the Hanse over the quality of wool, and the absence of any specifications for the best-quality English wool, suggest that a range of medium-grade wool was used; see 1470–71, Comptes des Argentiers, f° 71 r°, in de Pas, "Documents sur l'industrie drapière," 71–72. Moreover, regulations drawn up in 1464 refer to German-style *draps* in conjunction with cloth manufactured for domestic consumption, specifically textiles in the price range of 12 *sols parisis* per *aune*. These prices corresponded to those charged for retail versions of some of Saint-Omer's best cloth—both a light cloth of 1,400 threads and a somewhat heavier version of 1,600. While the export versions of these textiles were made with first-quality English wool, those made for domestic consumption were made with the best Flemish or medium-quality English wool; 1464, Saint-Omer, BB. 134.11, in d'Hermansaart, "Les anciennes communautés d'arts et métiers," 17: 176–85. These German-style cloths were referred to as *draps*, and Saint-Omer's *sayetterie* was defunct by the fourteenth century; Derville, "Draperies flamandes," 366–67. There is some indication, however, that these textiles were related to *says*. Saint-Omer's officials compared their city's German-style cloth to the textiles produced in Poperinghe, a village that concentrated on *say* production in the fourteenth and fifteenth centuries; see above, n. 58. And immigration to Saint-Omer from Poperinghe and Hondschoote, both of which specialized in *says*, increased during the time that Saint-Omer concentrated on making German-style cloth; 1452–53 to 1461–60, Saint-Omer, Archives Municipales, Comptes des Argentiers.

[72] Thirteenth and fourteenth-century assize lists show that Saint-Omer did not forbid foreign merchants from retail transactions in other types of cloth; 1250–1325 and *ca.* 1281, Saint-Omer, Bans Municipaux, in Espinas and Pirenne, *Recueil*, 267–68, 256.

[73] See 1250–1325, Saint-Omer, Bans Municipaux, in Espinas and Pirenne, *Recueil*, 239, 257.

[74] See 1453–54, Délibérations, B. f° xxi v°, and 1454–55, f° xxiv v°, in de Pas, "Documents sur l'industrie drapière," 62–63. This price is probably 12 *sols parisis*, as this is the type of money specified in the 1458–59 account. Twelve *sols* per *aune* seems to have marked the upper range of cloth produced specifically for domestic consumption; 1464, Saint-Omer, BB. 134.11, in d'Hermansaart, "Les anciennes communautés d'arts et métiers."

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unquestionably the more lucrative of the two, and the government allowed rural cloth on the retail market, so long as it was purchased for personal use only.[75] But opening the city's retail market to rural textiles did not solve the labor shortage. The city resorted to foreign labor from the countryside and other cities in order to meet the Hanse demand for cloth. Municipal deliberations during the boom give little evidence of this influx of population, but the role that foreign labor played becomes apparent in the months following the bust.

When the bust came, it hit Saint-Omer very hard. City leaders soon renewed the ban on the sale of moderately priced and inexpensive rural textiles in Saint-Omer's retail market. As part of the terms of the reimposition of the restriction, cloth manufacturers had to guarantee that they would produce a variety of textiles priced at 12 *sols* or less, like "the kind of cloth that is made outside of the city for poor people."[76] The cloth guilds had to promise that they would produce this cloth for the same price as that charged by rural producers. Additionally, in 1460, Saint-Omer's leaders attempted to support export-oriented production through the allocation of 2,000 livres for the purchase of English wool and its distribution to destitute weavers.[77] Nevertheless, neither the protection of the local market nor the subsidization of production for export could sustain a labor force swollen by over five years of intensified production for the Hanse. Within a few months, the city began to lose its cloth workers. Municipal deliberations show that, because of the severe fall in demand, cloth production was coming to a halt. Weavers, fullers, and shearers, as well as spinners, carders, combers, and "other workers and lesser folk [*menu peuple*] who earn their living from the production of cloth" quickly abandoned the city.[78] Governmental representatives went to the Hanse headquarters at Bruges to complain that the decrease in business, combined with the Hanse merchants' demand that the city sell its cloth to them on credit, had impoverished its cloth workers. The government wanted to know the German merchants' intentions in order to determine whether it should continue its efforts to maintain the cloth workers in anticipation of renewed Hanse patronage.[79] The news was not good. Admissions of new apprentices to the cloth

[75] See 1454–55, Saint-Omer, Délibérations, B. f° xxiv v°, in de Pas, "Documents sur l'industrie drapière," 63.

[76] "Item conclurent de communiquer avoec les trois mestiers et drappiers pour trouver manière de en ceste ville drapper draps de XII s. et en dessoubx de toutes sortes à tel pris que en les drappe au dehors de la ville pour subvenir au povre peuple"; 1457–58, Saint-Omer, Délibérations, B. f° xxxiv v°, and 1458, f° xxxv r°, in de Pas, "Documents sur l'industrie drapière," 63–64.

[77] "[C]onclurent de envoyer achetter laines d'Angleterre à Calais ou plus grant nombre que boinnement on pourra à créance et long jour de paiement, dont le ville se obligera vers les vendeurs, pour lesdites lains distribuer es mains des drappiers et bourgois à le drapper pour faire gaigner ledit povre peuple"; 1459–60, Saint-Omer, Délibérations, B. f° xliii v°, and f° xliv r°, in de Pas, "Documents sur l'industrie drapière," 67–68. The sum of 2,000 livres was in "deniers courans."

[78] The loss of Hanse patronage resulted in "dommage de ceulx qui ont fait et font drapper lesdits draps en telle manière que plusieurs ont cessé et cessent de plus en drapper et est moult diminuée, par quoy les trois mestiers, filleresses, pineresses [peigneuses], garderesses [cardeuses] et aultres ouvriers et le menu peuple qui se vivoit et estoit soubstenu en grant partie de ladite drapperie n'a de present aucune gaing mais les aucuns se sont déjà absentez"; 1458, Saint-Omer, Délibérations, B f° xxxv v°; see also 1459–60, f° xliii v°, and f° xliv r°, in de Pas, "Documents sur l'industrie drapière," 67–68.

[79] Delegates traveled to Bruges to meet with Hanse merchants "par quoy le povre peuple qui par cy devant s'estoit entremis de la drapperie, estoit fort apovry et en toute désolacion. Requérans savoir sur ce leur intencion, et s'ilz estoient en voulenté de débouter la drapperie de ceste dite ville adfin que

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guilds fell from twenty-two in 1456–1457 to two in 1459–1460 and remained depressed throughout the 1460s, although the indication is that, by 1464, Saint-Omer's trade with the Hanse did resume to the extent that municipal leaders again sought to guarantee a sufficient supply of cloth for domestic consumption.[80]

Undoubtedly, some of the cloth workers left for other cities. Saint-Omer belonged to the Hanse of the Seventeen Cities, a coalition that allowed master weavers to move to and work in any member city without requiring that they join its guild or citizenry.[81] Perhaps others left for the villages surrounding the city. Although the destinations of these workers are unknown, a few clues as to their previous origins are supplied by the lists of Saint-Omer's admissions to the bourgeoisie. The information provided by the admissions lists is very limited, because it only touches on those men who belonged to one of Saint-Omer's guilds and were prosperous enough to purchase citizenship.[82] The majority of the cloth workers who migrated to Saint-Omer between 1452 and 1458 belonged to the temporary work force and therefore did not become citizens. The names of cloth workers who did not belong to a guild, such as the women who prepared and spun the wool, never appeared on the lists, regardless of whether or not they became permanent residents of the city. Similarly, those who were not prosperous enough to purchase citizenship, such as the *menu peuple* who came to the city in hopes of finding employment during the boom, left no mark on the lists. And, although the lists of entries to the bourgeoisie indicate the place of origin of new citizens, they do not specify the actual date of immigration. Thus, at best, they can only suggest certain trends. Nonetheless, those trends reflected the boom of the 1450s. Entries to the bourgeoisie increased slightly during this period. The number of new apprentices admitted to the cloth guilds grew proportionately. The lists suggest that Saint-Omer was drawing on rural labor to produce cloth for the Hanse. The majority of the increase in admissions to Saint-Omer's bourgeoisie during the 1450s was supplied by nearby cloth-working villages in Flanders and Artois. The villages in Saint-Omer's immediate vicinity contributed the largest part of the increased immigration. Immigration from the cloth-producing villages and towns of Cassel, Bergues, and Hondschoote also grew at this time, as did that from the urban textile centers of Aire and Lille.[83] The cloth workers who did not appear in

les povres drappiers se peussent pourveir"; 1459, Saint-Omer, Délibérations, B. f° xlii r°; and Comptes des Argentiers, f° lxxvii r°, in de Pas, "Documents sur l'industrie drapière," 65–67.

[80] See 1456–57 to 1468–69, Saint-Omer, Archives Municipales, Comptes des Argentiers. A regulation in 1464 specified that every third cloth made in the German style had to be "sur le détail"; 1464, Saint-Omer, BB. 134.11, in d'Hermansaart, "Les anciennes communautés d'arts et métiers," 179.

[81] Vercauteren, "Note sur le survivance," 1078–91; Laurent, "Nouvelles recherches," 81–94. Saint-Omer restricted the right to weave cloth in the city to those who received their mastership in a "vile [*sic*] de loi"; 1306, Saint-Omer, Bans Municipaux, in Espinas and Pirenne, *Recueil*, 239, 257. And cloth workers who were banished from Saint-Omer were also prohibited from working in any of the other member cities; Laurent, *Un grand commerce*, 95, 240.

[82] See 1452–53 to 1459–60, Saint-Omer, Archives Municipaux, Comptes des Argentiers. Saint-Omer admitted few women to the bourgeoisie. Wives and daughters of citizens automatically received the privileges of citizenship, and their names were not recorded.

[83] Most of the increase in citizen entries occurred during the first three years of the boom; 1452–53 to 1459–60, Saint-Omer, Archives Municipales, Comptes des Argentiers.

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the citizenship lists, such as the spinners, carders, and combers, and those "lesser folk" who earned their living in some connection to the textile industry, appear in only a brief reference in a late thirteenth-century regulation, which suggests that, at least earlier, some of Saint-Omer's combers, spinners, and wool sorters came from the countryside.[84]

THE CASE OF SAINT-OMER'S CLOTH BOOM from 1452 to 1458 and the subsequent bust indicates that Saint-Omer relied on outside labor—both from other cities and from the countryside—and rural cloth production to meet a sudden increase in demand. That the city chose the strategies it did, such as opening the domestic market for cheap cloth to rural producers while seeking to preserve the more lucrative export market for its own craftsmen, and allowing outsiders to come work within the city, means that the relationship between town and countryside was not simply one of opposition or separation. The two industries were connected closely, and often the boundaries between them were fluid in spite of or, in the case of Saint-Omer in 1452–1458, because of urban regulations and privileges. The manipulation of these privileges and regulations implies that such economic institutions did not necessarily impose paralyzing rigidities on urban production. Rather, regulations could be used to facilitate the adjustment of urban manufacturing to rapid changes in demand while protecting the city's own valuable resources: its cloth workers and merchants. In this manner, at any given time, the productive potential of the city could be greater than its permanent artisanal population would indicate. Of course, these strategies also reveal the exposed position of many cloth workers in the later Middle Ages, who found it necessary to travel from city to city in search of work. For Saint-Omer's own craftsmen, however, the city offered protection during the boom as well as the bust.[85] Thus Saint-Omer's economic institutions of regulations, inspections, and guilds provided flexibility and the structure needed to protect its cloth industry.

Saint-Omer's cloth manufacturers undoubtedly competed with rural cloth producers, but the city and its countryside were more interdependent than competitive. Saint-Omer's relationship with the countryside conforms to the model of urban-rural relations that Hohenberg and Lees propose for proto-

[84] See 1275–1325, Saint-Omer, Bans Municipaux, in Giry, *Histoire de la ville de Saint-Omer*, 508. Some of the yarn undoubtedly was imported from the countryside; Pirenne, "Place of the Netherlands," 31.

[85] Hohenberg and Lees argue that preindustrial cities solved the problems of long-term economic fluctuations (in contrast to the short-term fluctuations discussed here) through immigration, marginalization, and mortality. The high mortality and low birth rates of immigrants and other members of marginal (non-citizen) groups allowed the urban population to expand or contract around a demographically stable core of permanent citizens. The authors acknowledge the possibility of emigration by this group but identify mortality as the key adjustment mechanism; *Making of Urban Europe*, 96–97. The adjustment of the urban population through mortality seems to be a very slow and inefficient adjustment mechanism more likely to create political and economic tensions than resolve them. Although the authors argue that cities needed a mechanism for adjusting population size to economic cycles, they do not make clear whether the brutal demographic consequences of restricted access to citizenship were recognized and consciously manipulated by those in power. The strategy consciously pursued by Saint-Omer's leaders in the 1450s and 1460s was clearly a more rapid adjustment mechanism to economic fluctuations.

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industrialism in France during the seventeenth and eighteenth centuries.[86] The case of Saint-Omer also supports Fujii's conclusion that the reciprocity and complementarity of urban and rural industry should lead to a reconsideration of these two categories of production.[87] In many cases, the distinction between rural cloth and Saint-Omer's (and a number of other cities') own product was a technicality that did not reflect the true state of textile production and trade in the late Middle Ages; rural cloth workers manufactured cloth in the city, and urban craftsmen finished cloth woven in the countryside.[88] Thus to assess the effect that rural textile manufacturing had on the urban economy by somehow subtracting the total number of pieces of rural cloth from the number made in the city would produce at best an inexact impression. Rural manufacturing served as a valuable resource for the city, creating a reservoir of skilled labor that allowed the city to sustain levels of production well above what its own permanent cloth-working population could provide. The presence of rural labor allowed cities a flexibility that would have been impossible to attain or sustain with urban resources alone.

Complementarity also best describes the role that urban markets played in rural cloth production. Cities found opportunities for profit in distributing rural-made cloth as well as the raw materials on which rural manufacturers depended. Moreover, urban markets were crucial to the widespread growth of rural manufacturing for export. The proliferation of rural textile centers necessarily increased the need for organization to coordinate the production and distribution of cloth, and the expansion of the regional-market functions of a number of the urban centers of Flanders and Artois must be seen in this light.[89] A few established markets, where buyers and sellers could gather to communicate market and price information, were undeniably more efficient than a host of tiny producers functioning independently of each other. Similarly, in the fifteenth century, the Hanse concentrated its market activity in a few cities. Concentration improved trading efficiency, and it gave Hanse merchants greater control over the production process.[90] It also required a tier of secondary markets where cloth was collected and transferred to one of the Hanse headquarters in the Low Countries. Saint-Omer became one of these collection and distribution centers and so did Lille.[91]

The case of Saint-Omer also indicates that the roles played by city and

[86] Hohenberg and Lees, *Making of Modern Europe*, 130–31.

[87] Fujii, "Draperie urbaine et draperie rurale," 91.

[88] For the fifteenth century, see above, n. 17. From the late thirteenth century, Saint-Omer allowed on its market cloth that was produced partly in the city and partly in the countryside. Urban regulations provided for the possibility of the transfer of rural cloth to the city at every stage of the production process, so long as the urban worker first submitted the cloth for inspection; *ca.* 1319 or 1279, and 1270, Saint-Omer, Bans Municipaux, in Espinas and Pirenne, *Recueil*, 253–54 and 238, respectively. Whether Saint-Omer continued the practice of finishing rural-made cloth during the course of the fourteenth century is unclear.

[89] I will discuss the reorganization of the market structure of this region in a future article, "Medieval Market Networks in the Later Middle Ages: The Case of Flanders and Artois."

[90] J. P. Peeters, "De-industrialization in the Small and Medium-Sized Towns in Brabant at the End of the Middle Ages: A Case Study; The Cloth Industry of Tienen," in van der Wee, *Rise and Decline*, 179.

[91] Bruges, in addition to serving as one of the Hanse headquarters, was a collection center for textiles made in the surrounding countryside; see above, nn. 18 and 20.

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countryside in the late medieval cloth industry were determined by more than just production or the cost of labor. Historians of proto-industrialization have long recognized that cities possessed advantages that favored them for certain kinds of production. Industries and processes requiring large concentrations of capital, such as dyeing, or a highly skilled work force, such as the manufacture of luxury woolens, remained in the cities. In addition to matters of production, the concentration of warehouses, market and transport facilities, and financial services ensured that cities also retained their commercial functions, such as the distribution of both urban and rural cloth.[92] To focus simply on the costs of production as the determinant factor in the location of manufacturing ignores some formidable obstacles in the way of market-oriented production at the end of the Middle Ages—in particular, difficulties in gaining information about market demand and prices; training, mobilizing, and coordinating labor; capital investment; and acquiring and distributing raw materials and finished products. Before we can determine the roles that city and countryside played in the manufacture and distribution of cloth at any given time, we must first ask where each of these functions (including each stage of manufacturing) took place and which of these functions acted as a bottleneck—in terms of costs, resources, organization, or training. The presence of a bottleneck at a stage in either production or distribution influenced the location of that stage (and possibly related stages). Such bottlenecks undoubtedly varied according to the type of cloth produced (for example, the manufacture of inexpensive cloth with little or no finishing would be subject to different bottlenecks than the production of expensive cloth, which required specialized finishing) and according to general economic conditions, such as the availability and cost of labor and raw materials, market demand, and the organization of the market economy.[93] Investigation of the conjuncture between bottlenecks in production and distribution may provide an explanation for the alternating fortunes of urban and rural cloth manufacturing in the medieval Netherlands. Such an investigation may also help explain the long-term shift northward and eastward in cloth production, from northern France and Flanders to Brabant and Holland.

Lastly, the part that Saint-Omer and other traditional urban textile centers played in the production and distribution of cloth should lead to a reconsideration of some of the assumptions that have been made when comparing the economic institutions and outlook of the traditional urban centers to the new trading and manufacturing centers that appeared during the later Middle Ages. In particular, it should call for a reconsideration of the functions performed by the economic institutions of medieval cities—their production regulations, privileges, and monopolies.[94] As can be seen in the case of Saint-Omer, these institutions could be

[92] Van der Wee writes of the deepening of urban human capital in the fifteenth and sixteenth-century Low Countries; "Structural Changes," 204, 213. See also van der Wee, "Industrial Dynamics," 329–30; and Hohenberg and Lees, *Making of Urban Europe*, 129.

[93] The influence of changes in population size and distribution, and of market demand on the location of textile manufacturing, has been discussed by Paul Klep, "Urban Decline in Brabant: The Traditionalization of Investments and Labour (1374–1806)," in van der Wee, *Rise and Decline*.

[94] For the early modern period, Richard Mackenney argues that historians are so convinced that guilds—another medieval institution—were obstacles to growth that they often overlook evidence to

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used to help change production and distribution in response to changes in demand. For instance, the role of production regulations in Saint-Omer's negotiations with the Hanse reveals a sensitivity to market demand, not a stubborn refusal to acknowledge it. Similarly, the city leaders' policy of alternately opening up the retail cloth market to rural producers and then reserving it for urban cloth manufacturers demonstrates that these institutions were flexible and that those in power were willing to adjust them to enhance the well-being of the city and to help adjust production to market demand. Likewise, market privileges and monopolies were highly selective. They did not necessarily eliminate all competition (which, in fact, they could not do outside of their own marketplace) or work against market forces. It is true that cities experiencing economic difficulties sought the enforcement of their production and market rights. The question is one of causation. Did market and production rights lead to decline, or did economic difficulties lead to a more rigorous insistence on these privileges? If the latter, then the assumption that the privileges were the cause of urban decline must be reconsidered. Once established, did they further exacerbate the city's economic difficulties?[95] The case of Saint-Omer does not support such an interpretation. Saint-Omer's privileges and regulations were selective and flexible, and they were able to accommodate the changes taking place in the late medieval market economy.

Clearly, privileges and production regulations did not foster free trade. Saint-Omer's leaders used them to exploit opportunities as fully as possible while protecting their own merchants and artisans. But such protection, associated with non-competitiveness and rigidity in Pirenne's model of urban-rural production, was not necessarily a rejection of developments in the market economy or of commercial capitalism. Rather, it could be an adaptation to them. Indeed, as protective measures, the privileges and production regulations insulated the cities and their precious investments in capital, skilled labor, and the market structure itself from the worst economic fluctuations. In a highly volatile economy, in which busts inevitably followed booms, and manufacturing centers were easily created and just as easily abandoned, the role of these institutions, acting like brakes, may have protected the city from the potentially destructive extremes of the late medieval market economy. These institutions undoubtedly slowed change, but they did not have to create rigidity. Indeed, in the case of Saint-Omer, there is considerable evidence that they ensured flexibility.

The rise of market-oriented manufacturing in the countryside was linked closely to urban cloth manufacturing and to the market networks developed by the old urban textile centers. Rural industry first appeared as a challenge when the urban textile centers' prosperity reached its height, not because cities were so inefficient but because they were part of the market network that could sustain

the contrary, such as their proliferation in the seventeenth century; Mackenney, *Tradesmen and Traders: The World of the Guilds in Venice and Europe, c. 1250–c.1650* (Totowa, N.J., 1987), 123–24.

[95] According to this position, guilds and other medieval urban economic institutions might not have been the original cause of decline, but, when decline set in, they made it impossible for cities to take the necessary measures for recovery. Such a position still identifies these institutions as forces of decline. See, for example, Gutmann, *Toward the Modern Economy*; and Peeters, "De-industrialization," 179.

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export-oriented manufacturing in the countryside. Even if rural textile production predominated at certain times, we cannot assume that this is proof of the modern economy having its roots in the country rather than the city, let alone of the countryside having some secret to success that the cities either could not or would not discover. Urban and rural cloth manufacturing were not two separate economic modes, the one hopelessly outdated and the other a blueprint for a modern economy. Indeed, the success of rural industry depended on the willingness of the cities to participate in the transformation of the medieval economy.

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The Absence of War in the U.S.-China Confrontation over Quemoy and Matsu in 1954–1955: Contingency, Luck, Deterrence?

GORDON H. CHANG and HE DI

KNOWN IN THE WEST AS THE QUEMOY-MATSU CRISIS OF 1954–1955, the dispute between the United States and the People's Republic of China over several small offshore islands held by the Chinese Nationalist government on Taiwan is often cited as one of the most intense confrontations of the early Cold War. For almost nine months, the American public feared that widespread military conflict, including the use of nuclear weapons, could break out at any time. After the crisis, American writers devoted considerable attention to its intensity, drama, and duration. They have studied it to understand U.S.-China relations, Dwight D. Eisenhower's presidential leadership, and the practice of deterrence, especially the doctrines of "brinksmanship" and "massive retaliation." Most American accounts have assumed that the Beijing leadership sought territorial expansion and have concluded that the Eisenhower administration's military resolve, deft political maneuvering, and public determination frustrated Beijing's ambitions, thus preventing widespread hostilities and even war. According to most American literature on this topic, Washington's handling of the crisis is a positive example of crisis management and of effective American conduct of the Cold War generally.[1]

This essay, one of the first collaborative efforts between an American historian and a scholar from the People's Republic of China, began in 1987 and struggled with the obstacles of political upheaval and distance. This essay draws from an expanded documentary base, but the authors also engaged in an interactive effort to seek common standards of argument, understanding about existing literature and interpretations, analytical assumptions, conceptual frameworks, methodologies, and expression. The final product is a genuine synthesis—it is impossible to determine who was responsible for which argument or conclusion. The authors wish to thank the Social Science Research Council (Committee on Foreign Policy Studies), Committee on International Relations Studies with the People's Republic of China, the Brookings Institution, and Stanford University's Center for International Security and Arms Control and Center for East Asian Studies for financial support that made this essay possible. The authors also express their appreciation for encouragement and for comments on earlier versions from A. Doak Barnett, Warren I. Cohen, Katherine de St. Paer, Alexander L. George, Harry Harding, Miles Kahler, John W. Lewis, Scott D. Sagan, Allen S. Whiting, and Xue Litai. We also thank Jing Li for her help with research.

[1] Most American accounts of the crisis have generally accepted Dwight D. Eisenhower's entire framework for understanding the crisis and his selection of issues for examination. See his presidential memoir *Mandate for Change: 1953–1956, The White House Years* (New York, 1963). Other extended discussions of the crisis in English can be found in the following works, Stephen E. Ambrose, *Eisenhower, Vol. II: The President* (New York, 1985); Robert Accinelli, "Eisenhower, Congress, and the 1954–55 Offshore Island Crisis," *Presidential Studies Quarterly*, 20 (Spring 1990): 329–48; Richard K. Betts, *Nuclear Blackmail and Nuclear Balance* (Washington, D.C., 1987); H. W. Brands, Jr., "Testing Massive Retaliation: Credibility and Crisis Management in the Taiwan Strait," *International Security*, 12 (Spring 1988): 124–51; McGeorge Bundy, *Danger and Survival: Choices about*

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American studies, however, have relied almost exclusively on U.S. documentary material and Western public sources and have only been able to infer the Chinese side of the story from Chinese published materials and observed behavior. On the other side, the standard Chinese public account of the 1950s, reflecting Beijing's official view of events and political purposes, has characterized U.S. China policy in the Taiwan Strait as one of inflexible hostility, aggressiveness, and warmongering.[2]

The following study contradicts much of these two established interpretations and is the first to use extensive archival material on the crisis from both China and the United States. Access to Chinese documentary material makes possible a fuller understanding of the conflict, as well as a reconceptualization of the entire confrontation. Through examining such documents, we are now able to gain a wider view of Chinese purposes and activity and thus evaluate the wisdom and effectiveness of both U.S. and Chinese policy making based on the consequences of policy and action, that is, on the interaction of U.S. and Chinese decision making. The Chinese evidence also raises new questions and issues and places U.S. documents in a substantially different interpretive light. (The authors of this essay together studied as much of the evidence as was available, but because access to some Chinese material remains restricted, the authors attempted to verify conclusions by using a variety of evidence and did not rely on a single source.)

In particular, this essay presents reassessments of seven principal issues related to the encounter: who initiated the crisis; Mao Zedong's purposes in the Taiwan Strait; Eisenhower's ambiguous policy, the Yijiangshan campaign, and the escalation of the crisis; China's attitude toward U.S. diplomatic efforts to end the crisis; U.S. interpretation of events in March and April 1955 and the climax of the

the Bomb in the First Fifty Years (New York, 1988); O. Edmund Clubb, "Formosa and the Offshore Islands in American Foreign Policy, 1950–1955, *Political Science Quarterly*, 74 (December 1959): 517–31; Michael Dockdrill, "Britain and the First Chinese Off-Shore Islands Crisis, 1954–55," in Dockdrill and John W. Young, eds. *British Foreign Policy, 1945–56* (London, 1989), 173–96; Robert A. Divine, *Eisenhower and the Cold War* (New York, 1981); Alexander George and Richard Smoke, *Deterrence in American Foreign Policy: Theory and Practice* (New York, 1974); Leonard H. D. Gordon, "United States Opposition to Use of Force in the Taiwan Strait, 1954–1962," *Journal of American History*, 72 (December 1985): 637–60; Morton H. Halperin and Tang Tsou, "United States Policy toward the Offshore Islands," *Public Policy*, 15 (1966): 119–38; Paul K. Huth, *Extended Deterrence and the Prevention of War* (New Haven, Conn., 1988); J. H. Kalicki, *The Pattern of Sino-American Crises: Political-Military Interactions in the 1950s* (London, 1975); Bennett C. Rushkoff, "Eisenhower, Dulles and the Quemoy-Matsu Crisis, 1954–1955," *Political Science Quarterly*, 96 (August 1981): 465–80; and Thomas E. Stolper, *China, Taiwan, and the Offshore Islands* (Armonk, N.Y., 1985). Also see the essays by Marc S. Gallicchio, Waldo Heinrichs, Rosemary Foot, Nancy Bernkopf Tucker, and others in Warren I. Cohen and Akira Iriye, eds., *The Great Powers in East Asia, 1953–1960* (New York, 1990); and the essays by Chen Xiaolu, Nancy Bernkopf Tucker, Wang Jisi, and Jia Qingguo in Harry Harding and Yuan Ming, eds., *Sino-American Relations, 1945–1955: A Joint Reassessment of a Critical Decade* (Wilmington, Del., 1989).

The authors have previously written on the offshore island crisis in Gordon H. Chang, *Friends and Enemies: The United States, China, and the Soviet Union, 1948–1972* (Stanford, Calif., 1990); and He Di, "The Evolution of the People's Republic of China's Policy toward the Offshore Islands," in Cohen and Iriye, *Great Powers*.

[2] For example, see Han Nianlong, *et al.*, eds., *Diplomacy of Contemporary China*, Qiu Ke'an, *et al.*, trans. (Hong Kong, 1990). This is a translation of the semi-official *Dangdai Zhongguo Waijiao* (Beijing, 1989). A recent article in the official *Beijing Review* on the offshore island crises reflects the use of much of the Chinese documentary material consulted for this essay and familiarity with current American scholarship; see Li Yuanchao, "The Politics of Artillery Shelling: A Study of the Taiwan Strait Crises," *Beijing Review* (September 7–13, 1992): 32–38; also see Xu Yan, *Jinmen Zhi Zhan* (Beijing, 1992).

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crisis; the end of the confrontation; and leadership performance during the crisis. Mutual misunderstanding about intentions and objectives in the confrontation was exceedingly high between the two countries (and remained so for forty years), and much of the historical interpretation on both sides has been deeply misleading. This essay, as part of an emerging post–Cold War historiography that draws on Communist sources, suggests that some important assumptions and even lessons drawn from the conduct of the Cold War, especially in the area of security studies, may require rethinking as we begin to learn more about "the other side."[3]

EISENHOWER AND MOST AMERICAN HISTORIANS have given September 3, 1954, when Chinese Communist shore batteries opened fire on the Nationalist-held offshore island of Quemoy, as the beginning of a crisis that lasted for almost nine months.[4] Most Western accounts have assumed that the Communist leadership ordered the shelling as part of a centrally directed military campaign that was at least a probe of the strength of the U.S. security commitment to Quemoy, if not the beginning of an actual effort to seize the island. Washington reacted to the shelling by dramatically increasing the U.S. military presence in the Taiwan Strait, strengthening Nationalist defenses, and issuing increasingly stern warnings to Beijing over the following months. To bolster its commitment to the Nationalists, the Eisenhower administration completed negotiation of a mutual defense treaty and received a blank check from Congress in early 1955, the so-called Formosa Resolution, for the use of American forces to defend Taiwan island and the nearby Pescadores.

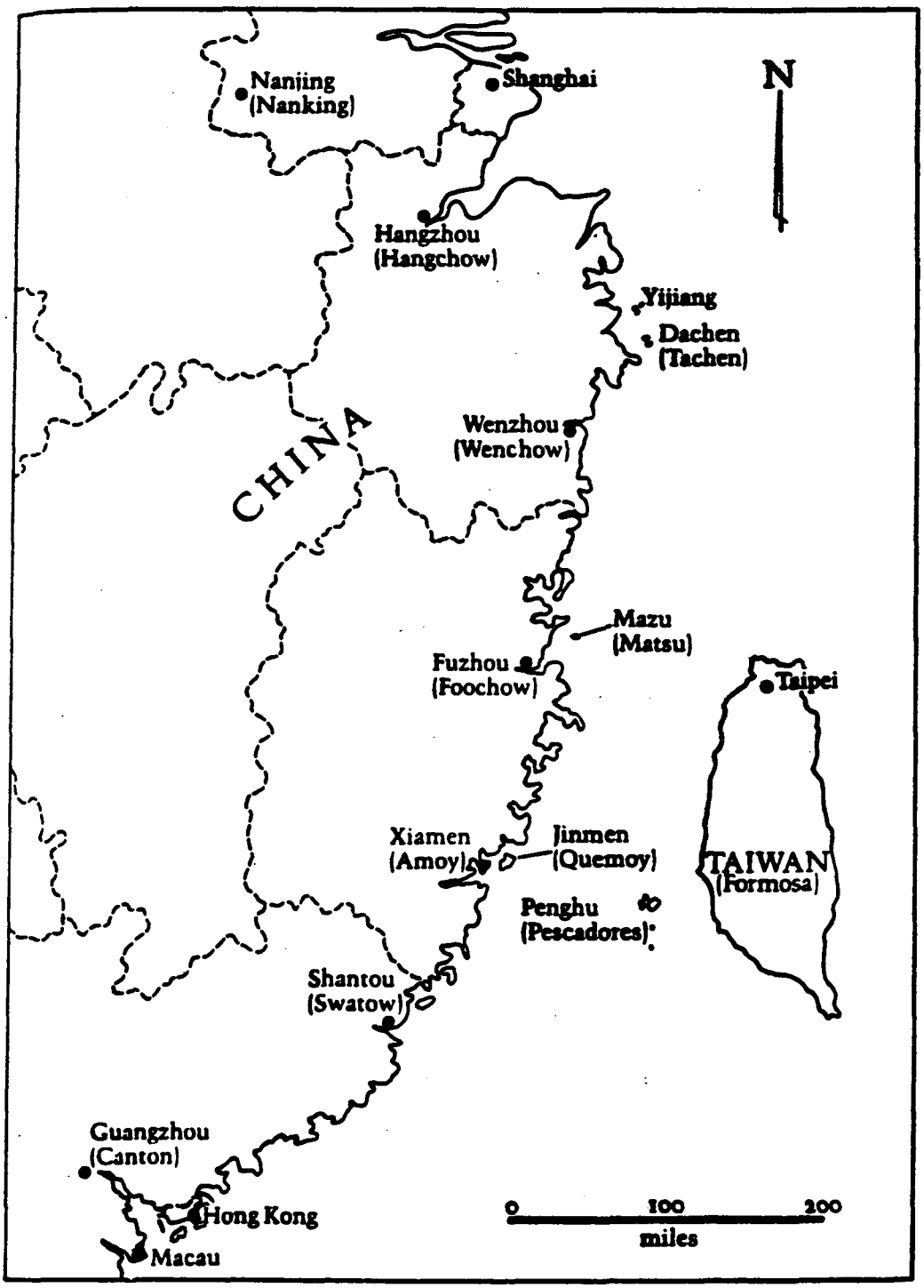
The Chinese documentary record of high-level decision making at that time, however, does not indicate that Beijing considered September 3 shelling a precipitant event or even an unintentional initiation of a confrontation with the United States. Furthermore, the Chinese documentary record does not show that China's leaders considered the period from September 1954 to late April 1955 to be especially tense. In contrast to former U.S. officials who vividly recollected the main events, high-level Communist officials and advisers active during the 1950s and interviewed for this essay did not recall the September 3 bombardment or consider the time one of sharp conflict with America.[5]

[3] See, for example, James G. Blight and David A. Welch, *On the Brink: Americans and Soviets Reexamine the Cuban Missile Crisis* (New York, 1989); and Sergei N. Goncharov, John W. Lewis, and Xue Li-tai, *Uncertain Partners: Stalin, Mao, and the Korean War* (Stanford, Calif., forthcoming, 1994).

[4] The significance of September 3 for the United States is also reflected in the documentary record. See *Foreign Relations of the United States* (hereafter, *FRUS*), 1952–54 (Washington, D.C., 1985), 14: 556–58; Central Intelligence Agency (CIA), "The Chinese Offshore Islands," September 8, 1954, Formosa (1), Box 9, International Series, Eisenhower (AW), Dwight D. Eisenhower Library, Abilene, Kansas; *New York Times* (September 5, 1954): 1. We understand a "crisis" as being a distinct period of escalating international tension with a high possibility of the outbreak of large-scale military conflict.

[5] See Han, *Diplomacy of Contemporary China*, 94–100, 131–37; interviews with Chinese officials; Han Huaizhi, *et al.*, *Dangdai Zhongguo Jundui de Junshi Gongzuo* (Beijing, 1988), 254–71, 384–418; Yang Guoyu, *et al.*, *Dangdai Zhongguo Haijun* (Beijing, 1987), 189–223. The issue concerning the term "crisis" is substantive and not semantic: Chinese documentary evidence and current semi-official accounts indicate that China's leadership considered the 1958 confrontation over the offshore islands a "crisis," with a distinct beginning and conclusion and a high level of tension. This essay refers to the 1954–1955 confrontation as a crisis, since at least one party, the United States, saw it as one and acted accordingly.

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Offshore islands in the Taiwan Strait. Copyright Stanford University Press.

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Instead, the Chinese documentary record indicates that China's leaders, and local commanders of the People's Liberation Army (PLA) in particular, considered the events of these nine months as little different in nature from what had transpired in the region since 1949 and not as a separate campaign; that is, they understood the events as an integral part of the continuing tensions of the Chinese civil war in the offshore area. The Nationalists had retained control of some thirty offshore islands, many just a few miles from shore, when they retreated from the mainland in 1949. "The civil war against Chiang Kai-shek never ended in the coastal regions," maintained a high-level, internal report of the Chinese Communist Party (CCP) on July 24, 1954.[6]

Because Nationalist and Communist military forces had frequently clashed in the southeastern China coastal area during and after the Korean War in 1953 and 1954, Beijing did not consider the September 3 shelling, though dramatic, to be a radical departure from the pattern or level of hostilities in the area. Other observers at the time, including forces friendly to the United States, also evaluated the September 3 shelling of Quemoy, and the subsequent events in the offshore island area, in a different light than did Washington. The British government saw the activity as part of the latest round of feuding between the Nationalists and the Communists that had broken out during the summer of 1954, and it was not convinced that the Communists intended to attack Quemoy or were even principally interested in the island.[7] The Nationalist military high command also expressed in private its belief that the Communists were only probing U.S. intentions with the shelling and were not about to launch an all-out assault on Quemoy. On September 10, the Chinese Nationalist Party organ, *Central Daily News*, dismissed the mainland's "Liberate Taiwan" campaign, which began in late July 1954, as simple propaganda and the Quemoy shelling as part of a political, rather than military, campaign.[8]

[6] Chinese Communist Party (CCP) Central Committee Propaganda Department and the Political Department of the Central Military Commission, "Guanyu Taiwan Wenti de Junshi Baodao de Zhishi," July 24, 1954, Zhongyang Dang'anguan, comp., *Zhonggong Zhongyang Wenjian Huibian: 1954* (hereafter, *Wenjian Huibian*), 2722–24. Also see Wang Yan, *Mubiao—Yijiangshan* (Beijing, 1990), 54; Han Huaizhi, *Dangdai Zhongguo Jundui*, 256; interviews with Zhang Aiping, Ye Fei, Lei Yingfu; research into restricted archives.

[7] Humphrey Trevelyan to Allen, January 22, 1955, FO371/115038/FC1041/445, Public Records Office, Kew, England; ALUSNA, Taipei, to Secretary of State, September 3, 1954, 793.00/9–354, Record Group 59, National Archives, Washington, D.C.; Dockdrill, "Britain and the First Chinese Off-Shore Islands Crisis, 1954–55," 173–96. Also see Zhai Qiang, "Britain, the United States, and the Jinmen-Mazu Crises 1954–55 and 1958," *Chinese Historians*, 2 (Fall 1992): 25–48.

[8] The *Central Daily News* reported that the Communist shelling killed three Nationalist soldiers and wounded seven others, but it also expressed full confidence in the capability of the Nationalist army. The purposes of the mainland's "Liberate Taiwan" political campaign, according to the newspaper, were to influence the UN, to influence the neutral nations of Asia, to make trouble for the United States, to divert attention from domestic unrest, and to split the free world; September 4, 1954, p. 1; September 5, p. 1; September 10, p. 6. Chiang Kai-shek also drew attention to the political purpose of Beijing's attack on the offshore islands. "The purpose of the Chinese Communist attack on the offshore islands is to undermine the U.S.-Taiwan Mutual Defense Treaty and the western Pacific security system." See Zhu, *et al.*, *Zhonghua Minguo Shishi Jiyao*, 1954 (Taipei, 1989), 301. Although Chiang raised the issue of Quemoy with John Foster Dulles during a discussion in Taibei on September 9, 1954, Chiang spent most of the time pressing Dulles for progress toward the treaty and U.S. support for a Nationalist "counter-offensive against the mainland." Chiang expressed no concern about an imminent loss of territory but spoke repeatedly about taking the initiative against the Communists. Shen, translated Memorandum of September 9 conversation between Chiang and

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Why, then, did the Eisenhower administration mistakenly assess the situation and consider the shelling the start of a deliberate military confrontation? In September 1954, U.S. antipathy toward the Chinese Communists and fear of their international ambitions ran high in the aftermath of the Korean War and the Vietnamese Communist defeat of the French in Indochina during the spring of 1954. The United States had steadily increased its attention to the South China Sea and China mainland offshore area for some time before September 3. U.S. ambassador to the Nationalists, Karl Rankin, and military intelligence had both warned of the possibility of trouble, including the danger of a Communist attack on Quemoy, months earlier.[9] And two U.S. men in uniform, members of the Military Assistance Advisory Group, had been killed in the Communist bombardment, which was more serious than previous sporadic shelling.

In addition, and perhaps more important, the Eisenhower administration became alarmed because the September 3 attack apparently demonstrated the failure of previous U.S. efforts at deterrence. In the spring and summer of 1954, the United States had twice sent ships of the Seventh Fleet to the Dachens (offshore islands along the Zhejiang coast) in a show of force to impress the Communists. As late as August 20, less than two weeks before the Quemoy shelling, Secretary of State John Foster Dulles sent a strong message of reassurance to the U.S. ambassador to Japan, who had communicated to Washington his own and the Japanese government's worries about possible Communist military activity in the offshore area. Dulles pointed out that because of his recent article in *Foreign Affairs*, which advanced the doctrine that later became known as "massive retaliation," he was certain that Beijing and Moscow fully appreciated the U.S. resolve to oppose Communist aggression. "I do not think that the Japanese need be alarmed," he wrote in his confidential letter, "because I do not believe that the Chinese Communists are in fact now prepared to challenge us in any major or sustained way and provoke further our sea and air power along their coast." In an August 24 press conference, Dulles pointedly warned the Communists that an attack on Nationalist-held offshore islands might provoke a U.S. armed response.[10]

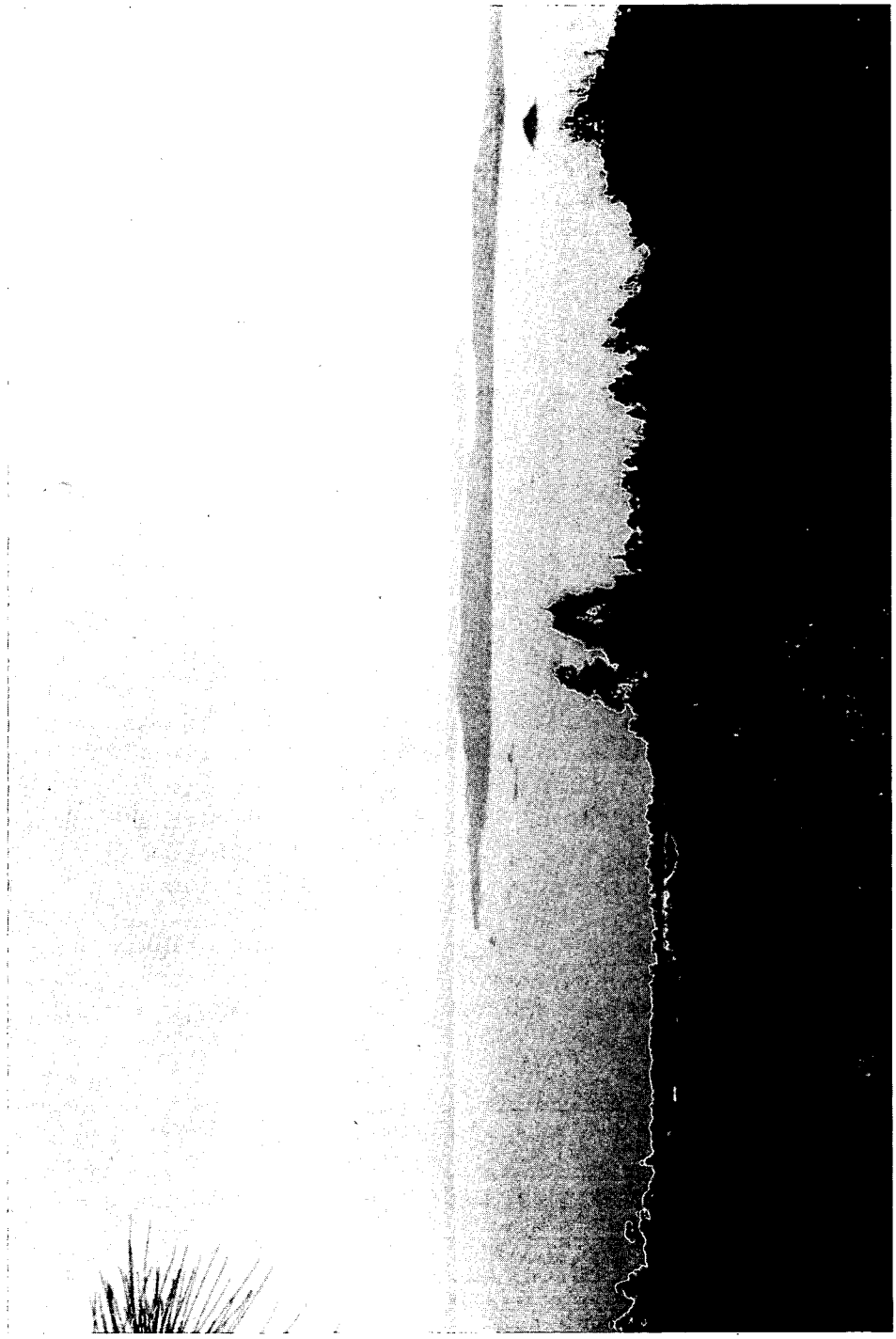
Thus the September 3 shelling most likely came as a rude shock for Washington, considering Dulles's expressed confidence in the power of U.S. deterrent force. Assistant Secretary of State for Far Eastern Affairs Walter Robertson

Dulles, attached to Karl Rankin to State Department, September 16, 1954, Chiang Kai-shek—1954, Box 23, Karl Rankin Papers, Seely Mudd Library, Princeton University. The U.S. ambassador on Taiwan reported to Washington on September 5 that he refused to "take the present fracas around Kinmen very seriously from a purely military standpoint"; Karl Rankin, Offshore Islands Chronology, June 3, 1955, Re: Offshore Islands, Box 28, Rankin Papers. Rankin, in his words, expressed "many times the opinion that there probably would be no major Communist assault" after the September 3 shelling because Beijing did not have the military capability. Karl Rankin, Memorandum of conversation with Chiang Kai-shek and others, May 4, 1955, Re: Chiang Kai-shek, Box 26, Rankin Papers.

[9] *FRUS*, 1952–54, 14: 392–94, 412–14, 415–16.

[10] *FRUS*, 1952–54, 14: 545–47. Also see Brands, "Testing Massive Retaliation." American case studies that take September 3, 1954, as the start of U.S. deterrent efforts begin, therefore, with an incorrect premise. Also see Richard Ned Lebow and Janice Gross Stein, *When Does Deterrence Succeed and How Do We Know?* (Ottawa, 1990).

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Two of the Nationalist-held offshore islands just a few miles from the Chinese mainland. In the distance is Quemoy, with "Little Quemoy" closer to shore. The foreground is an area near the Chinese city of Xiamen. Observers using binoculars can easily see the Nationalist flag and fortifications from the mainland. The authors took the picture during a trip to the Fujian coastal region in the summer of 1993.

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explicitly interpreted events as a failure of deterrence in a September 4 memo to Dulles, arguing that the shelling had proved earlier U.S. efforts could "no longer be relied upon to deter Communist attacks" and that "more positive action by the United States [was] necessary if these [offshore] islands are not to be swallowed up by the Communists." Specifically, Robertson pressed for direct U.S. involvement in the event of a Communist assault on a major offshore island.[11]

WHAT WAS THE ACTUAL PURPOSE of the shelling that so disturbed U.S. officials? The memoir of General Ye Fei, the Communist commander of the Fujian forces responsible for the bombardment of Quemoy, reveals that orders Beijing sent on August 25 instructed Ye Fei to shell Quemoy, not as a preliminary to an assault on the island but as a specific and limited response to what was perceived as an increase in U.S. and Nationalist military provocations in the area and the rumored negotiation of a mutual defense treaty between Washington and Taibei. It was Ye Fei personally, not the central authorities, who recommended September 3 for the shelling for the simple reason, General Ye believed, that the Nationalists planned to supply the island by ship on that day. Communist shelling of Quemoy after September 3 in 1954 and 1955 was, in fact, infrequent and light.[12]

Supporting the contention that the Chinese did not consider the September 3 shelling a prelude to a coordinated assault on Quemoy, or even as a serious probe of the U.S. commitment to defend the island, are the recollections of Chinese officials and of the Communist military commanders in charge of forces in the offshore island area at the time. In addition to Ye Fei (who was also the field commander of a failed attack in October 1949 on Quemoy and commander of the PLA Navy in the 1980s), General Zhang Aiping, commander of the Zhejiang forces (minister of defense in the 1980s), and Lei Yingfu, military secretary to Zhou Enlai and an adviser to Mao Zedong, both independently confirm that the only military plan developed at the time was one to take the Dachens, an offshore island group far to the north of Quemoy and Matsu held by the Nationalists; the plan included neither Quemoy nor Matsu.[13]

[11] *FRUS*, 1952–54, 14: 561–63.

[12] Ye Fei, *Huiyilu* (Beijing, 1988), 643; Kalicki, *Patterns of Sino-American Crises*, 130. The CIA speculated that the shelling may have been linked to the signing of the SEATO (South-East Asian Treaty Organization) treaty. CIA, "Chinese Offshore Islands." The shelling may also have served as a diversion from the mainland's campaign against the northern offshore islands; David G. Muller, Jr., *China as a Maritime Power* (Boulder, Colo., 1983), 24; He Di, "Evolution of the People's Republic of China Policy toward Jinmen and Mazu," 224; and Chen Xiaolu, "China's Policy toward the United States, 1949–1955," in Harding and Yuan Ming, *Sino-American Relations*, 194.

[13] Interviews with Zhang Aiping, Ye Fei, and Lei Yingfu.The recollections of the Chinese officials are supported by a CIA intelligence report issued several days after the September 3 shelling. According to the CIA, "Communist naval and ground force activity in the [Quemoy] area" was "not unusual," and they had given no indication "they were readying their forces for an attempted invasion of the Quemoys." The report also confirmed that Chinese naval and air capabilities in the Quemoy area were extremely poor, with no railway link to the Fujian coast nor jet fighters "close enough to provide effective cover for operations in the Quemoy area." CIA, "Chinese Offshore Islands." On September 15, 1954, the State Department's Office of Chinese Affairs summarized the intelligence information it had and concluded that "there is no evidence of a significant build-up" of landing craft around Quemoy, "no Communist air activity in the area," and "all sources agree that there is now no indication that a Communist attack on the Chinmen [Quemoy] islands is imminent";

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The order to Ye Fei reflected the thinking of Mao Zedong, China's commander-in-chief, about the Taiwan Strait. Convinced that the United States fully endorsed the Nationalist harassment of the mainland, Mao held Washington responsible for the mounting tensions in the area before September 1954 and firmly believed that Beijing was the defender, not the aggressor, in the Strait. Isolated in the international community, with relatively limited information, and with Leninist assumptions about the relationship of imperialism and semi-colonies, Mao could not know that there were serious strains in the U.S.-Nationalist relationship or that Washington, uneasy about the Nationalist activities, wanted to limit Chiang Kai-shek's attacks on mainland forces. He mistakenly assumed that Chiang was little more than a puppet of the United States. In addition, Mao questioned the sincerity of Washington's professed desire to reduce tensions with the Communist world after the 1954 Geneva Conference. As a result, he concluded that China had to respond to the perceived U.S.-Nationalist provocations that occurred in late 1953 and early 1954.[14]

Mao was especially worried that a U.S.-Nationalist mutual defense treaty, reports of which began to circulate in China in mid-1954, would play a role similar to that of the cease-fire in Korea and the Geneva agreement on Vietnam, which had formalized the division of those two close neighbors of China.[15] He had no idea that substantial differences existed between the Eisenhower administration and Chiang over the proposed treaty. Thus, on July 23, Mao sent a telegram to Zhou Enlai, who was en route from Geneva to Beijing, which admonished,

> in order to break up the collaboration between the United States and Chiang and to keep them from joining military and political forces, we must announce to our country and the world the slogan of the Liberation of Taiwan. It was improper of us not to raise the slogan in a timely manner after the cease-fire in Korea. If we were to continue dragging our heels now, we would be making a serious political mistake.[16]

After Zhou's return to China, Mao convened a political bureau meeting at the leadership retreat at Beidaihe, where he presented, in his typically grand style, general guidelines for a propaganda campaign for the Liberation of Taiwan, the first such campaign against Taiwan in the history of the People's Republic.

In Mao's view, the conflict with the Nationalists in the Strait occurred on political, military, diplomatic, and propaganda fronts. Politically, acquisition of

Stryker to Walter Robertson, September 15, 1954, Chinese Nationalist Troops on the Offshore Islands, Box 48, RG 59, Office of Chinese Affairs 1948–1956, National Archives.

We do not exclude the possibility that included in the calculations of Mao and other central Chinese leaders was an interest in seeing the Nationalist and American reaction to a shelling of Quemoy. Indeed, if the Nationalists had simply abandoned Quemoy, the Communists would surely have occupied it. But the variety of evidence we have consulted shows that Beijing developed no operational plan to assault Quemoy linked to the September 3 shelling or anytime afterward in 1954 and 1955.

[14] Zhou Enlai, "Jingji Jianshe de Jige Fangzhenxing Wenti," November 10, 1956, *Zhou Enlai Xuanji, II* (Beijing, 1984), 236.

[15] *FRUS*, 1952–54, 14: 343–44; interviews with Pu Shouchang and Lei Yingfu. Zhou Enlai's statement on the Mutual Defense Treaty, December 8, 1954, *Zhong-Mei Guanxi Cailiao II* (Beijing, 1961), 2077–81.

[16] Wang Bingnan, *Zhongmei Huitan Jiunian Huigu* (Beijing, 1985), 5–6.

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Taiwan was central to the unification of Chinese territory, and China could not accept any treaty arrangement between the United States and the Nationalists that formally separated Taiwan from the mainland or established an independent status for Taiwan. Diplomatically, the conflict was part of the struggle with the United States and a test of whether Washington's announced intention to relax tensions with the socialist camp was real. Militarily, the People's Liberation Army could respond to Taiwan's military harassment, gain control over the Dachen offshore islands, and use the opportunity to train its forces. In terms of domestic propaganda, the campaign for Taiwan would help rekindle the enthusiasm of the Chinese people for New China after the conclusion of the Korean and Vietnam conflicts. The country would have the slogan "We Must Liberate Taiwan" to rally around, providing both a goal for which to strive and a new external enemy to oppose.[17]

In line with Mao's instructions, the Propaganda Department of the Central Committee and the Political Department of the Central Military Commission formally initiated a massive "Liberate Taiwan" campaign in late July 1954. It developed through three stages and decisively influenced Washington's interpretation of the subsequent events in the offshore area. A July 23 *People's Daily* editorial entitled "We Must Liberate Taiwan!" marked the start of the campaign. Propaganda for the next several months aimed at rallying the people on the mainland around the central slogan and lasted until mid-December. The campaign entered a second and more intense stage on December 13, when the Central Committee issued a further directive condemning the successful negotiation of the Mutual Defense Treaty a few days earlier and a U.S. military build-up in the offshore area. The third stage began in February, when the Central Committee circulated "Instructions on the Nationwide Petition Campaign against Nuclear Weapons" and "Instructions on Propaganda concerning U.S. Interference in the Question of Our Liberation of Taiwan," in response to the U.S. Congress's Formosa Resolution and U.S. nuclear threats.[18] The overall aim of the propaganda campaign was to present China's position on Taiwan to the world and mobilize the Chinese people, but it was the bombardment of Quemoy in the midst of this campaign that attracted most international notice.

Mao Zedong wanted this attention. Over the years, he had accumulated much experience in using controlled military action for discrete political purposes. He often used his armed forces to raise tensions and to dramatize his political position, such as in the offensives of the People's Liberation Army in northeast

[17] Interviews with Lei Yingfu and Pu Shouchang.

[18] During the propaganda campaign, the CCP Central Committee issued instructions to its cadres at different levels. The main directives, all found in *Wenjian Huibian, 1954* and *1955*, are: the CCP Propaganda Department and Political Department of the Central Military Commission, "Guanyu Taiwan Wenti de Junshi Baodao de Zhishi," July 24, 1954, 2722–24; CCP Central Committee, "Guanyu Jiefang Taiwan de Xuanchuan Fangzhen de Zhishi," September 25, 1954, 1711–19; CCP CC, "Guanyu Kaizhou Fandui Meijiang Tiaoyue Yundong de Zhishi," December 13, 1954, 120–24; CCP CC, "Guanyu Chuanda Zhou Enlai Tongzhi Zhengzhi Baogao de Tongzhi," December 25, 1954, 1131–37; CCP CC, "Guanyu zai Chuenguo Kaizhan Fandui Shiyong Yuanziwuqi Qianming Yundong de Zhishi," February 9, 1955, 436–37; CCP CC, "Guanyu Meiguo Ganshe Woguo Jiefang Taiwan Wenti de Xuanchuan Tongzhi," February 21, 1955, 536–43; CCP CC, "Guanyu Zhou Enlai Tongzhi he Yingguo Zhuwoguo Daban Trevelyan Tanhua Neirong," February 28, 1955, 573–79.

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China during George C. Marshall's mission to China in 1945–1946 and during the armistice negotiations in Korea. The bombardment of Quemoy was exactly such a political-military demonstration, as the Chinese Nationalist observers on Taiwan had correctly surmised in the *Central Daily News*. Large-scale "armed propaganda" might be another description of the effort. The shelling was part of Mao's attempt to focus world attention on the Taiwan issue and what he believed was U.S. interference in Chinese internal affairs.

In contrast to the shelling of Quemoy for political purposes, Mao saw the campaign to take the Dachen Islands as essentially a military operation. To be clear, Mao saw the two theaters, Quemoy and the Dachens (which were under two separate military commands), in very different ways. He considered the shelling of Quemoy to be essentially political and low risk, since he believed he could easily control the action and avoid widescale conflict in the region, as there would be no direct military contact between Communist and U.S. personnel. The assault on the Dachens was another matter. Mao estimated that it carried a higher risk, since the possibility of direct clashes with U.S. armed forces was much greater. Thus Mao and the Chinese central command, unlike the Eisenhower administration, paid almost no attention to Quemoy but closely followed the Dachens campaign. The Central Committee's confidential instruction on the Liberate Taiwan campaign, circulated on July 24, stated, "At present, the direct target of our military struggle is Chiang Kai-shek and his cohorts in Taiwan. The United States should not be treated as our direct target; we should confine the conflicts with the United States to the diplomatic arena only."[19]

As for the relationship of the activities directed against the offshore islands to the military liberation of Taiwan, China's leaders were under no illusion that they could soon successfully assault Chiang's main island fortress. In a leadership directive on September 25, the CCP Central Committee observed that the capture of Taiwan was a "long-term and complex struggle" and was a strategic rather than immediate task. "We are not able to liberate Taiwan without a powerful navy and air force and need time to build them up." A Central Committee instruction in late February 1955 stated, "we will be able gradually to complete the historical task of the liberation of Taiwan through a long-term struggle as we grow in strength and achieve victories one by one in the military, political, and diplomatic fields." The assumption about the protracted nature of the task of taking Taiwan was repeated in subsequent Central Committee instructions. The January 1955 assault on Yijiangshan Island, a small, vulnerable target related to the Dachens, taxed China's amphibious capabilities to the limit and required the largest combined force operation to date in Chinese Communist history. The Commu-

[19] CCP Propaganda Department and Political Department of the Central Military Commission, "Guanyu Taiwan Wenti de Junshi Baodao de Zhishi," July 24, 1954. Mao issued orders concerning the Dachens campaign on June 2, July 24, August 21, December 11, 1954, and February 2, 1955, but he virtually ignored Quemoy. We have found no other instructions from Mao for the bombardment of Quemoy except the directive of August 23.

Mao repeatedly restrained his military commanders in the Dachens area. See his cables of June 2, July 23, July 24, August 21, December 11, 1954, and February 2, 1955, ordering them not to attack the Dachens if U.S. forces were in the area. Zhonggong Zhongyang Wenxian Yanjiushi, ed., *Jianguo Yilai Mao Zedong Wengao, 1954* (Beijing, 1990), 495, 516–17, 520, 533, 627; *1955*, 23.

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nists posed no military threat, either in immediate intention or capability, to Taiwan.[20]

EISENHOWER, ASSUMING THAT THE COMMUNISTS did present a military challenge, described his own policy in the offshore area after the September 3 shelling as one of "keeping the enemy guessing" whether the United States would actually involve itself in a battle over Quemoy and Matsu. Dulles later aptly described this policy in private as deterrence through uncertainty.[21] This strategy was adopted for several reasons, which included Washington's own questions about overall Communist objectives, Eisenhower's insistence on avoiding rigid commitments and maintaining flexibility of action, and administration concern about the lack of domestic and international support for further U.S. involvement in the offshore island area. At the heart of the matter, though, was the U.S. dilemma that the president, while supporting Chiang on Taiwan, did not want to be pinned down to a commitment of indefinite length to any of the offshore islands, over a hundred miles away from Taiwan and insignificant militarily in his eyes.

Eisenhower's ambiguous policy was controversial at the time and remains so. In private, Dulles, other high-level officials, and some foreign governments repeatedly differed with the president's policy and recommended that the United States publicly clarify whether or not it would fight for Quemoy and Matsu. Dulles and others frequently argued within the administration that an unambiguous position was necessary: the Communists would then have no basis from which to miscalculate and launch an attack under the erroneous belief that the United States would not involve itself in the actual defense of Quemoy and Matsu.[22] Chinese documentary evidence now shows that Eisenhower's deterrence through uncertainty did indeed confuse the Communists. In fact, it eventually encouraged

[20] CCP, "Guanyu Meiguo Ganshe Woguo," February 21, 1955, and "Guanyu Jiefang Taiwan," September 25, 1954; Han, *Dangdai Zhongguo Jundui*, 254; Nie Fengzhi, *et al.*, *Sanjun Huige Zhan Donghai* (Beijing, 1985), 39–56. Nie was the commander of the Eastern Air Force, deputy commander of the East Zhejiang Frontline Headquarters, and in charge of the Dachens campaign. A study by a U.S. naval officer and historian argues that, contrary to assumptions that the mainland constituted a genuine military threat to Taiwan in the 1950s, "no such threat in fact existed." China built not one of the amphibious landing ships that would have been necessary for an invasion of Taiwan during the 1950s and "developed none of [the other] essential elements to the extent required to conduct an opposed amphibious invasion"; Muller, *China as a Maritime Power*, 52–56.

[21] Meeting of the Secretary of State with Congressional Leaders, January 20, 1955, Formosa Straits (3), White House Memoranda 1955, Box 2, White House Memoranda Series, John Foster Dulles Papers, Eisenhower Library. Dulles told the Congressmen, "Up to the present time we have been covering this situation [in the offshore area] by hoping the communists would be deterred by uncertainty. They are probing and will continue to probe to find where we will stop them."

[22] See "Draft Memorandum for the President," February 1955, Offshore Islands 1955, Box 53, Office of Chinese Affairs, 1948–1956, RG 59, National Archives; *FRUS*, 1955–57, 2: 41–44, 69–82, 83–85, 86–89, 111–12, 114–15, 173–76, 400, 451–52. Praise for Eisenhower's ambiguous policy comes from Divine, *Eisenhower and the Cold War*; and Ambrose, *Eisenhower, Vol. II: The President*, 245. The U.S. stance was, in fact, doubly precarious: its declared position was vague, and after the evacuation of the Dachens it secretly pledged to Chiang that it would involve itself in a fight over Quemoy and Matsu. The United States therefore accepted all the risks and responsibilities but accrued none of the assumed advantages of a policy of deterrence.

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Beijing to launch the assault on Yijiangshan, a major unintended consequence of which was, in turn, a dramatic U.S. escalation of tension in the spring of 1955.

In Beijing, as we indicated above, while Mao supported the military commanders' original plan to take the Dachens Islands along the Zhejiang coast, he wanted to limit military operations in the interests of broader political considerations, such as not alienating world public opinion and avoiding military clashes with U.S. forces, which would risk failure and lead to domestic morale problems. Thus, even though Mao considered the liberation of the offshore islands an internal affair of China's, one of his principal concerns in his decision to authorize the assault on Yijiangshan was whether the United States would come to its defense. On August 21, 1954, Mao instructed Liu Shaoqi, Zhou Enlai, Chen Yun, Deng Xiaoping, Peng Dehuai, and Huang Kecheng (deputy chief of staff): "Please note, only after verifying that there are no U.S. ships or planes present can we launch the attack on the Dachens. Otherwise, do not initiate any military action."[23]

The local Communist military commanders viewed the situation in the Strait with different interests in mind. They were concerned primarily with regional and operational issues rather than the wider political context. For example, their attention was devoted to coastal security, the protection of fisheries and other ocean resources, defense of sea lanes, the improvement and expansion of their military capability, and the morale and readiness of the forces under their commands. Still smarting from their defeat by the Nationalists in a bloody 1949 assault on Quemoy, they were eager for new military action and pressed central authorities to support their proposals, in particular, the campaign to take the Dachens.[24]

Mao had to restrain his local commanders repeatedly in late 1954 and early 1955 to prevent military clashes with the United States. He strictly instructed the People's Liberation Army not to provoke U.S. forces and to guard against even accidental conflicts with them.[25] PLA planes were ordered not to fly into international territorial areas and their pilots were not to engage U.S. planes without obtaining express prior approval from the Eastern Air Force Headquarters.[26] Mao authorized the army to attack Nationalist troops but directed the scale and timing of the campaign according to his estimate of U.S. intentions and commitments.

If Washington, instead of avoiding explicit commitments to the defense of the offshore islands, had consistently demonstrated its determination to defend the islands, Mao would not have been likely to approve the assault on the Dachens. For example, in the middle of July 1954, the Central Military Commission ordered the Yijiangshan campaign to begin in September or October, and on August 10, the East Military Headquarters approved preparations for the actual attack on Yijiangshan. On August 20, the Headquarters asked the Central Military Commission to approve the assault for sometime between September 1

[23] Mao, "Remarks on the Timing of the Attack on the Dachen Islands" (August 21, 1954), *Jianguo Yilai, 1954*, 533.

[24] Interviews with Zhang Aiping, military commander of the Zhejiang region, 1988; Wang Yan, *Mubiao*, 54, Nie, *Sanjun*, 2.

[25] Han, *Dangdai Zhongguo Jundui*, 256–57; Nie, *Sanjun*, 16; Mao, *Jianguo Yilai, 1954*, 495, 516–17.

[26] Nie, *Sanjun*, 16.

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and 5. But, on August 21, when Mao learned of the high level of American attention to the area, he ordered that the campaign start only when there were no U.S. ships and aircraft present.[27] In this instance, U.S. military deterrence was successful; its naval presence raised the strong possibility of direct American involvement and made the Communists pause. The attack was delayed for several months.

But U.S. deterrent efforts were not consistent. In early December, release of the terms of the Mutual Defense Treaty revealed that its provisions expressly covered only Taiwan and the Pesacadores, not the Dachens and other offshore islands.[28] Before the treaty's provisions were known and because of the vagueness of the Eisenhower administration's position, Mao had wondered whether Washington would directly involve itself in combat over the offshore islands; after the disclosure of the treaty terms, he and his military commanders concluded that the United States would not join in active defense, since the treaty omitted specific mention of the offshore islands. He therefore allowed the commanders to go ahead with their military campaign. Mao drew conclusions directly opposite to those Eisenhower and Dulles had hoped to encourage with the treaty.[29]

Mao nevertheless proceeded cautiously. On November 30, just before the release of the terms of the Mutual Defense Treaty, Su Yu, the chief of staff of the People's Liberation Army, ordered the Yijiangshan attack to start around December 20. But, on December 11, because of a new U.S. naval maneuver in the Dachens, Mao countermanded Su Yu's order and again postponed the campaign for a month.[30] Even on the eve of the assault on Yijiangshan, the central leaders in Beijing considered further delaying the operation, as they worried about U.S. intervention if the campaign did not end quickly. Beijing asked General Zhang Aiping if he could guarantee success in the operation, and if he could not, he was to wait longer. Zhang, however, argued that "the arrow was in the bow" and had to be shot. He maintained that the United States, according to his intelligence sources, would not become involved; that technical and morale considerations required action; that it would not be possible to keep the plan to take Yijiangshan secret much longer; and that he had the "right" to select the specific timing for a local campaign. His insistence moved Mao to compromise and, thus, he deferred

[27] Wang Yan, *Mubiao*, 48; Mao, "Remarks on the Timing of the Attack on the Dachen Islands," August 21, 1954, *Jianguo Yilai, 1954*: 533.

[28] The Eisenhower administration believed that the legal status of Taiwan and the Pescadores was different than that of the offshore islands. The former had been colonies of Japan for fifty years, while the offshore islands had always remained under Chinese jurisdiction. The administration also wanted to keep open the option of an independent Taiwan. See Chang, *Friends and Enemies*, 116–17, 121, 147–48.

[29] Dulles and Nationalist Foreign Minister George Yeh initialed the Mutual Defense Treaty on December 2, 1954, and Eisenhower signed it into law in mid-February, after its ratification by the Senate. It formally committed the United States to the defense of Taiwan and the Pescadores Islands. Its provisions could be extended to other unnamed territories upon agreement of both signatories. Some Eisenhower administration officials were aware that the treaty, without covering the offshore islands, might "give a green light to the Communists to take the offshore islands"; *FRUS*, 1952–54, 14: 750, 827–39. Dulles hoped he could "fuzz up" the language in the treaty "to maintain doubt in the minds of the Communists as to how the U.S. would react to an attack on the offshore islands"; *FRUS*, 1952–54, 14: 828–29; Zhang Aiping interview; Wang Yan, *Mubiao*, 67; Nie, *Sanjun*, 50.

[30] Wang Yan, *Mubiao*, 53; "Report from the PLA East China Military Region Headquarters to the Central Military Commission," December 9, 1954, Chinese Communist Party Central Military Archives, Beijing; Mao, "Remarks on the Timing of the Attack on Yijiangshan Island," December 11, 1954, *Jianguo Yilai, 1954*, 627.

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to Peng Dehuai, head of the army, to make the final decision. Peng approved Zhang's request, and the assault took place the next day. On January 18, 10,000 PLA troops in the People's Republic's first large-scale coordinated air, sea, and land operation overwhelmed 1,086 Kuomingtang soldiers on Yijiangshan, inflicting heavy losses.[31]

On February 8, Chiang, under mounting pressure from the United States, announced he would withdraw from the vulnerable Dachens, which he did with U.S. help on February 12. In a major operation, U.S. ships evacuated 14,000 civilians and 10,000 Nationalist troops with their equipment from the islands;[32] and People's Liberation Army troops, without firing a single shot, then occupied them and the Nanji, the last of the Zhejiang offshore islands. While the quick and smooth military victories achieved by the People's Liberation Army greatly encouraged and emboldened some local commanders (they wanted to continue and expand the campaign to include bombing Taiwan itself), Mao stopped such thinking and criticized their proposals as adventurist. What is more, he came to conclude that while the Zhejiang coastal operations had been military successes, they had been political failures, since they attracted more U.S. attention than expected and raised the danger of direct military conflict with the United States.[33] Mao decided it was time to try to reduce tensions in the region; he did not want military operations to press any further.

Mao was right about the adverse consequences of the Dachens campaign. The capture of the Zhejiang islands by the People's Liberation Army led Washington to conclude that the Communists, rather than ending their "Liberate Taiwan" campaign, were intent on escalating the crisis and perhaps even assaulting Taiwan itself.[34] The subsequent dramatic U.S. response, in turn, increased tensions. Ironically, Mao believed that he had taken a cautious and restrained path, while U.S. leaders concluded that China's leaders were aggressive and bent on war. As Eisenhower wrote in his diary on March 26, "the Red Chinese appear to be completely reckless, arrogant, possibly overconfident, and completely indifferent as to human losses."[35]

Rather than deterring Communist action, Eisenhower's policy of keeping the enemy guessing had sent mixed signals to Beijing, which contributed to the Communist decision to assault Yijiangshan. For his part, Mao had underestimated the effect Beijing's actions and propaganda would have on Washington. Although attentive to the U.S. reaction, he had little sense of the extreme measures Eisenhower officials were then discussing (such as widespread bombing of Chinese industrial and military facilities and the use of nuclear weapons) as a response to what Washington believed was China's preparation for large-scale

[31] Interview with Zhang Aiping; Wang Yan, *Mubiao*, 120–22, 202; Han, *Dangdai Zhongguo Jundui*, 264–65, 270; Nie, *Sanjun*, 51; Zhu, *et al.*, *Zhonghua Minguo Jiyao, 1955*, 129–30. Some 519 Nationalist troops were killed, with another 567 captured. The Communist casualties were 393 killed and 1,024 wounded.

[32] Briscoe to Eisenhower, February 12, 1955, 091 China (February–March 1955), Box 6, Admiral Radford, 1953–1957, Records of the Joint Chiefs of Staff, RG 218, National Archives.

[33] Interviews with Zhang Aiping, Ye Fei, and Lei Yingfu.

[34] *FRUS*, 1955–57, 2: 279–86, 307–10.

[35] Eisenhower, *Mandate for Change*, 478.

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hostilities in the Strait.[36] By misinterpreting each other's intentions and signals, both sides, in taking what each considered to be prudent and justifiable actions, contributed to an increasingly dangerous situation.

IN ADDITION TO DISPLAYING MILITARY SUPPORT for the Nationalists and making other deterrent efforts, the United States also energetically explored diplomatic means of negotiating an end to the crisis.[37] But these diplomatic efforts, which advocated an in-place cease-fire, failed to resolve the conflict. The reason usually cited by Western specialists is that both the Nationalists and Communists rejected third party solutions because neither wanted outside involvement in what was seen as an internal matter. Both believed that efforts by a third party such as the United Nations would violate Chinese sovereignty, perpetuate a mutually undesirable status quo, and create a precedent for acceptance of a "two China policy" and permanent separation of Taiwan from the mainland—unacceptable to Communists and Nationalists alike. In addition, American writers point out that Eisenhower faced strong domestic political pressures, from those who pressed for disengagement from the offshore islands as well as from China Lobby forces, which campaigned for explicit commitments to the retention of the offshore islands. These political pressures complicated U.S. diplomatic efforts.[38]

But factors on the Chinese Communist side, of which Eisenhower was apparently not aware or to which he was not entirely sensitive, also made efforts at a negotiated approach problematic, if not virtually impossible. Washington had taken what it considered were serious measures to meet what it perceived as China's military challenge in the Strait, such as making shows of force there, strengthening the Nationalist defenses on the offshore islands, and issuing repeated warnings to the Communists.[39] Nevertheless, by February 1955, Mao concluded that Washington would not go to war over the offshore islands, and therefore the enhanced U.S. military posture was primarily for psychological purposes. A Central Committee directive issued on February 21, 1955, just after the Nationalist evacuation of the Dachens, reflected this attitude. It argued that China's actions had forced Eisenhower to give up for the moment his position of supporting Chiang's aim to "invade the mainland and restore the old order" and return to a policy similar to the Truman administration's defensive posture on China. The directive described that policy as one of "drawing a cease-fire line" to protect Chiang's occupation of Taiwan. Since conditions were not in place for the United States to start hostilities, the directive observed, China could continue to agitate against the United States and the Nationalists without provoking war.[40]

Beijing also concluded that the non-coercive efforts pursued by Washington indicated it was not ready to engage again in direct, widespread military conflict with China. These efforts included having third parties such as Britain, Sweden,

[36] Chang, *Friends and Enemies*, 131–37.
[37] See Gordon, "United States Opposition to Use of Force."
[38] See Accinelli, "Eisenhower, Congress, and the 1954–55 Offshore Islands Crisis."
[39] On "coercive diplomacy," see Alexander L. George, ed., *Avoiding War: Problems of Crisis Management* (Boulder, Colo., 1991), 384–87.
[40] CCP Central Committee, "Guanyu Meiguo Ganshe."

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and India urge China and Taiwan to take their dispute to the UN; having the Nationalists withdraw from the Dachens with U.S. help; and urging China through the UN and various other channels to accept a cease-fire proposal. Not only Mao interpreted Washington's behavior as contradictory and as sending mixed signals; U.S. ambassador Karl Rankin also made this point in a personal note to assistant secretary Walter Robertson after the Nationalists withdrew from the Dachens. "It is almost impossible," he wrote, "to overestimate the danger of confirming the Reds in a belief that, despite recent strong statements by the Secretary and others, we are for peace at any price. Withdrawal from the Dachens undoubtedly strengthened them in this belief." To correct this misimpression, Rankin advocated a military engagement with the Communists to convince "the enemy that we mean business."[41] Rankin had accurately anticipated Mao's reaction—U.S. diplomatic efforts were undercutting the credibility of its coercive efforts.

Since the United States did not pose a genuine threat to China in Mao's eyes, Beijing could continue to keep pressure on to split the allied camp and weaken the main enemy, the United States, without risking widespread conflict. As a Central Committee comment on British-Chinese relations put it, maintaining the campaign against Taiwan would "enlarge the contradiction between England and the United States."[42]

The effort to divide the enemy camp was part of Mao's version of "brinksmanship," what Dulles described as his own policy of averting war by going to the edge of war to intimidate one's adversary. Mao had a similar view of war and war avoidance, reflected in the CCP's instruction titled "U.S. Interference in the Question of Our Liberation of Taiwan," issued on February 21, while Dulles was on tour in Asia. "Regarding Washington's call for a 'cease-fire' and its threat to start a war," the instruction pointed out, "if we show any fear, the enemy will consider us weak and easy to bully. In other words, if we give them an inch, they will take a mile and intensify their military expansion. Only by adopting an unyielding, resolute, and calm stance can we force the enemy to retreat." In its interpretation of the lessons of history, the Chinese Communist Party observed that the September 18th Incident, when Japan invaded Manchuria in 1931, and the 1938 Munich compromise showed that peace cannot be obtained through appeasement: "Therefore, we must adopt an intransigent stance against the United States."[43] As part of that stance, Beijing rejected Washington's diplomatic efforts, advanced formal diplomatic proposals that it knew were unacceptable to the United States (although, at the same time, Zhou Enlai quietly suggested direct

[41] *FRUS*, 1955–57, 2: 360–64. Also see Marc Gallicchio, "The Best Defense Is a Good Offense: The Evolution of American Strategy in East Asia, 1953–1960," in Cohen and Iriye, *Great Powers*, 75.

[42] CCP Central Committee, "Guanyu Zhou Enlai Tongzhi." The *Central Daily News* had ascertained that one of China's purposes in the Liberate Taiwan campaign was to isolate the United States in Asia and disrupt U.S.-British cooperation (September 10, 1955): 6. On U.S.-British differences over the offshore islands, see Dockdrill, "Britain and the First Chinese Off-Shore Islands Crisis"; and Rosemary Foot, "The Search for a *Modus Vivendi*: Anglo-American Relations and China Policy in the Eisenhower Era," in Cohen and Iriye, *Great Powers*, 143–63.

[43] Zhou Enlai's outline of the political report to the First Plenary Session of the Second Chinese People's Political Consultative Conference, *Zhong Mei Guanxi Cailiao II*, 139. Since both adversaries had their own versions of brinksmanship, the Strait crisis was doubly dangerous.

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bilateral talks between the United States and China; his initiative will be discussed below), and persisted with a strident anti-U.S. campaign and a "Liberate Taiwan" campaign at home, which was one of Mao's main interests in the first place.[44]

Thus Mao's own brinksmanship and China's inflexible declared position had little to do with "maintaining face," as Eisenhower once argued in an effort to explain Chiang's and "Oriental" behavior during the crisis.[45] Instead, the perceived absence of a fully credible U.S. threat, Beijing's own tactical considerations, and concern for Chinese domestic politics all contributed to frustrating the diplomatic efforts of the United States. Eisenhower's dismissal of the diplomatic stalemate as being a result of Chinese preoccupation with "face" and of fanaticism was obviously too simple and avoided confronting the political complexity of the dispute between the Nationalists and Communists and the sophistication of the tactics each Chinese side employed.

AFTER THE PEOPLE'S LIBERATION ARMY ENDED ITS LARGE-SCALE MILITARY OPERATIONS off the Zhejiang coast with the taking of the Dachens, the situation in the Taiwan Strait should have stabilized. Dulles himself observed on March 9 that there was a "lull" in activity in the Strait, and some U.S. intelligence reports noted that the amount of belligerent Communist propaganda in late February and early March had fallen to its lowest point since the summer of 1954. The Eisenhower administration, however, again misjudged the situation and concluded that the Communists were continuing their preparation for aggression. Dulles saw the lull not as Beijing's effort to lessen tensions but as just the opposite. He concluded that the Communists were engaged in "a large-scale build up." It was this misassessment that led the United States to escalate tensions in mid-March, and the crisis lurched toward open hostilities.[46]

Mao was partially responsible for Dulles's perception, since Mao wanted to keep the pressure on the United States, which he did through propaganda; and Washington officials could not clearly distinguish between what was propaganda and what was an expression of genuine intent. This lack of clarity likely contributed to Dulles's revised estimate of the seriousness of the military situation during his trip to Asia in late February and early March 1955. While in Asia, he concluded that the United States was then contending militarily with China for the fate of Taiwan itself.[47]

Dulles's alarm, however, appears to have been unfounded and was based on inaccurate intelligence reports that grossly overestimated the ability of the

[44] Beijing refused to accept either the New Zealand cease-fire proposal or the UN invitation to open negotiations. It also declined British and other third country offers to mediate. In the meantime, Moscow, in consultation with Beijing, suggested that the UN discuss the "U.S. invasion of Taiwan," and on February 4 the Soviet Union proposed a ten-nation conference, including China, the United States, Britain, France, the Soviet Union, and India, to discuss the crisis in the Taiwan Strait at a conference in Shanghai.

[45] *FRUS*, 1955–57, 2: 285.

[46] *FRUS*, 1955–57, 2: 339–41, 319–20; Armstrong to the Under Secretary of State, July 13, 1955, Offshore Islands 1955, Box 53, Office of Chinese Affairs, 1948–1956, RG 59, National Archives.

[47] *FRUS*, 1955–57, 2: 299–300, 307–10, 314–15, 345–50, 352–53.

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People's Liberation Army to attack Quemoy, let alone Taiwan. According to Chinese documentary sources, the army had no air and sea force in the region except in Zhejiang, and it employed only two regiments of artillery troops in the shelling of Quemoy in September. These regiments even had to change their positions after the bombardment to create confusion and avoid a punishing Nationalist counterattack.[48] Zhang Aiping and Ye Fei also point out that China's local military capability was so limited that commanders could only plan for one small field campaign at a time; Zhang Aiping recalls that it took him months to assemble a motley assortment of 144 landing craft from along the entire east China coast (Shantung to Fujian) for the Yijiangshan assault. Ye Fei maintains that a campaign against Quemoy was simply impossible, since there was not even a railway link to Fujian's coastal area near the offshore islands. Railway construction started in the spring of 1955 and was not completed until April 1957. Construction of airstrips for jets in Fujian began in October 1954 but was not completed until May 1956.[49]

Dulles seems to have been deeply affected by fears he heard repeatedly expressed during his Asian trip about the danger of war in the Strait. Burmese Prime Minister U Nu, for example, told Dulles that a December visit to Beijing convinced him that the Communists were going to attack Taiwan. He reached this conclusion after personally witnessing the furious propaganda campaign against the Mutual Defense Treaty and the United States, the high point of the campaign occurring coincidentally during his visit. Dulles was also in contact with British Foreign Minister Anthony Eden, who had been trying to work with Zhou Enlai to find a peaceful settlement to the crisis. Eden's efforts, however, had fallen victim to Beijing's tactic of keeping pressure on Washington's allies and had proved futile.[50]

These encounters, combined with the lack of any progress in a UN diplomatic solution and Beijing's own rigid public position, apparently convinced Dulles that the Communists were bent on aggression. They were preparing for an offensive against Quemoy and eventually Taiwan, he believed, and the United States had to take decisive action to forestall being dragged into a conflict based on China's terms and China's timing. Thus the administration began to think increasingly

[48] Ye, *Huiyilu*, 349–50.

[49] Interviews with Zhang Aiping, Ye Fei, and Lei Yingfu. Allen S. Whiting, a low-level U.S. official who was in Taiwan, Hong Kong, and Japan during the crisis, recalls that "at no time did the atmosphere among our close associates in the [Hong Kong] Consulate or the experienced foreign correspondent community give any cause for concern over the prospect of war. The consensus held the concern was all in and from Washington, not Beijing"; Whiting to He Di, personal communication, February 3, 1992. Other high-level U.S. intelligence reports at the time were not consistent with Dulles's alarm: on the basis of U.S. and Nationalist intelligence, including aerial photographs of the areas around Quemoy and Matsu, Chief of Naval Operations Robert B. Carney concluded in Taibei on March 6 that there was "nothing very noteworthy of interest to Washington" in the tactical situation in the offshore area and that "there appears to be very little evidence of ChiCom air build-up as yet, except at the large field in the vicinity of Tachen." The report did not change Dulles's expectation that "severe fighting" in the Strait would soon break out; *FRUS*, 1955–57, 2: 329–36, 339–43; Andrew J. Goodpaster, Memorandum for the Record, March 18, 1955, Formosa Visit to CINCPAC (1955) (2), Box 9, International Series, Eisenhower (AW); Rankin, Memorandum of Conversation with Chiang Kai-shek and Others, May 4, 1955, Re: Chiang Kai-shek, Box 26, Rankin Papers.

[50] *FRUS*, 1955–57, 2: 339–43, 341 n. 4, 345–50.

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about preemptive military options to end the threat, including the use of nuclear weapons against Communist capabilities in the offshore area. The administration's alarm was reflected in dramatic White House discussions on March 10 and 11 during which the use of military force, including nuclear weapons, was extensively discussed and in the administration's repeated public warnings that it was prepared to use nuclear weapons against China. On March 16, Eisenhower himself frightened an American public by stating at a press conference that he saw no reason why nuclear weapons could not be used "as you use a bullet or anything else," in the event of war in the Taiwan Strait.[51]

Even though Eisenhower complained about "conflicting intelligence information he was receiving" at one point during the March 11 National Security Council meeting, the administration assumed the worst of China because of its deep-seated hostility toward, and suspicion of, Beijing.[52] Chairman of the Joint Chiefs of Staff Admiral Arthur Radford expressed the logical consequence of this attitude when he argued at a March 24 Security Council meeting that just because the United States had "so little operation intelligence regarding the Chinese Communist build-up" for an assault on the offshore islands, the United States should not assume that such a build-up was not proceeding. "Accordingly," Radford maintained, the United States "must assume that the Chinese Communists are getting ready just as fast as they can."[53]

The administration's belief that the situation required decisive steps led to the formulation of a secret Eisenhower-Dulles plan that included Chiang's withdrawal from Quemoy and Matsu, a U.S. blockade of five hundred miles of China's coastal waters opposite Taiwan, and the stationing of nuclear weapons on Taiwan so long as Beijing called for the "liberation" of the island. At the climax of the crisis, Eisenhower sent Radford and Robertson to present the proposal to Chiang in mid-April, but Chiang, not wanting to surrender any further territory and not fully trusting Washington, rejected it.[54]

Neither Beijing nor Washington had wanted direct conflict, and Chiang's acceptance of the evacuation and blockade plan would have brought the situation precipitously close to inadvertent war. As a provocative "fait accompli," the

[51] *FRUS* 1955–57, 2: 345–60. Also see Chang, *Friends and Enemies*, 126–28; and Brands, "Testing Massive Retaliation," 253.

[52] Robert Cutler, Memorandum for the Record, March 11, 1955, Formosa Straits (2), White House Memoranda 1955, Box 2, White House Memoranda Series, Dulles Papers. Dulles himself also admitted to the National Security Council at its March 10 meeting that "U.S. intelligence material had not been too good"; *FRUS*, 1955–57, 2: 345–50.

[53] *FRUS*, 1955–57, 2: 391. In *Extended Deterrence*, Paul Huth, arguing that the Communists did want to assault Quemoy and Matsu, cites U.S. intelligence reports that noted the improvement of military capabilities in the Fujian area; 104–15. As we said previously, however, American intelligence contained conflicting information. Eisenhower himself on March 28 mentioned to his press secretary that there was no build-up near the offshore islands to sustain an attack; *FRUS*, 1955–57, 2: 408–09.

[54] The content of the U.S. proposal to Chiang was not revealed to the press, although there was public speculation that the United States wanted the Nationalists to abandon Quemoy and Matsu. The U.S. officials, according to the *Central Daily News*, had come to Taiwan to discuss "active," not "passive," strategies in the Strait. The two men personally visited Quemoy during their trip; April 25, 27, and 28, 1955, all p. 1. By April, U.S. military officials were pressing the administration for authorization to allow Nationalist air strikes against Communist targets in the area opposite Taiwan. See *FRUS*, 1955–57, 2: 465–66, 490, 493–94.

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blockade would have invited Communist retaliation, including clashes between U.S. and Chinese forces. Even Radford himself anticipated such a turn of events. The proposal "meant war," the U.S. ambassador to Taiwan remarked when he heard the details of the plan, just before it was presented to Chiang.[55]

AMERICAN SPECIALISTS SUGGEST that Chinese Premier Zhou Enlai delivered his famous statement at the Bandung Conference in late April 1955 about China's peaceful intentions as a direct response to Washington's coercive efforts and that it constitutes evidence of the effectiveness of Dulles's threat of "massive retaliation."[56] It hardly seemed coincidental that Zhou spoke at virtually the same moment that Radford and Robertson, known to be hard-liners against China, were in Taiwan talking with Chiang. Soon after Zhou offered his remarks, however, Dulles confidentially told associates that "diplomacy and not merely force" had played a large part in producing Zhou's statement. Dulles claimed that U.S. political work with Asian allies had led China to "follow a pacific rather than belligerent course."[57] It appears that Dulles's surmise was right, at least partly. Zhou's speech was an impromptu response to discussions he had had with Asian delegates at Bandung, but it was neither specifically planned by the Beijing leadership before Bandung nor presented as a response to U.S. nuclear threats.

In early April 1955, before Zhou Enlai's departure for Bandung, Indonesia, the political bureau of the Chinese Communist Party met to discuss general policy toward the conference. Party leaders at this meeting decided that China's delegation was to seek common ground between China and other Asian countries and keep differences to a minimum. Zhou was to try to establish new relationships with China's neighbors based on the recently formulated five principles of peaceful coexistence. In order to achieve this goal, the Chinese delegation decided that they would not raise controversial issues, including the Taiwan question, at the conference. Zhou was given full power to handle the situation during the meeting, which was held from April 18 to 24.[58]

Despite Zhou's desire to avoid discussing Taiwan, and although he barely mentioned the issue publicly at the conference, many Asian leaders privately asked Zhou about the situation in the Strait. Finally, the day before the meeting's conclusion, toward the end of a luncheon with a number of Asian delegation leaders at the residence of the Indonesian prime minister, Zhou was again asked about the tensions in the offshore area. Zhou responded that China distinguished

[55] *FRUS*, 1955–57, 2: 529–30. In Chang, *Friends and Enemies*, it is argued that the outbreak of war came dangerously close during the crisis. The new evidence showing that China had no plans to attack Quemoy or Matsu suggests that the threat of war may not have been as great as previously assessed. However, Mao's sensitivity to the domestic consequences of the offshore island campaign and the eagerness with which his military commanders wanted to press the campaign against the Nationalists strengthens the contention that the central Communist leadership would have likely forcefully challenged a U.S. blockade of Chinese waters.

[56] See Dulles's statement in James Shepley, "How Dulles Averted War," *Life* (January 16, 1956): 70–80.

[57] *FRUS*, 1955–57, 2: 526–28; Dulles, Memorandum of Conversation with the President, August 5, 1955, Meetings with the President 1955 (3), Box 3, White House Memoranda Series, Dulles Papers.

[58] Han, *Diplomacy of Contemporary China*, 98–100; interview with Pu Shouchang.

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the conflict between the mainland and Taiwan from that between China and the United States. Taiwan was an internal question and linked to the Chinese civil war, he stressed, but the tension between China and the United States was an international matter, which China was willing to discuss with Washington. Zhou's off-the-cuff conciliatory comments surprised many of those present, who were urged not to speak publicly about them to avoid creating misunderstandings. Zhou was also asked, however, if he would make a general public statement, and later in the afternoon of the same day, China's delegation issued a brief press release containing his views. The haphazard manner by which Zhou's comments were delivered indicate that the presentation of his proposal was unplanned. Zhou himself believed his statement reflected general policy that had already been expressed, that adhered to Mao's point of view, and that was not a departure from China's previous position.[59]

Upon learning of Zhou's remarks, the Eisenhower administration responded positively to what it considered to be a significant step on China's part, and tensions quickly subsided in the offshore island area. Although the build-up of military capabilities by both the Nationalists and Communists continued, direct talks between Washington and Beijing soon began.[60]

Why did the United States take particular note of Zhou's comments at Bandung and not of other earlier messages indicating China's interest in negotiating directly with the United States? A positive U.S. response to previous Chinese signals might have prevented the crisis from escalating to the dangerous level that it did in late April. On February 5 and 9, after the Communists had taken Yijiangshan, Zhou had met with the Swedish ambassador to China, acting on behalf of UN Secretary General Dag Hammarskjöld, and had expressed China's interest in direct discussions with the United States regarding the Taiwan Strait. The content of these discussions, including Hammarskjöld's personal evaluation that Zhou's statements and manner were not "of a man intent on wild adventures" and that Zhou sincerely appeared to desire negotiation, had been communicated to Washington by Henry Cabot Lodge, U.S. ambassador to the UN, on February 6 and again by the U.S. deputy representative on February 11. No evidence is available, though, that Washington took seriously these or other earlier offers by the People's Republic to engage in systematic talks with the United States.[61]

There may be several ways to explain the U.S. behavior. First, Zhou's previous

[59] *Ibid.* Zhou's report to Mao on Taiwan Issue during Bandung Conference on April 30, 1955, Central Archives of the CCP, Beijing; Han, *Diplomacy of Contemporary China*, 98–100. Also see the perceptive, first-hand observations in A. Doak Barnett, "Chou En-lai at Bandung: Chinese Communist Diplomacy at the Asian-African Conference," May 4, 1955, American Universities Field Staff Report, Indonesia, ADB-4-'55; and Jia Qing-Guo, "Searching for Peaceful Coexistence and Territorial Integrity," in Harding and Yuan, *Sino-American Relations*, 267–86. While some Asian observers were surprised by Zhou's comments, India's Krishna Menon told Dulles a week after Zhou spoke that his statement "did not present [a] new attitude" but was what Menon had described to Dulles in late March; *FRUS*, 1955–57, 14: 536–38, 392–93.

[60] Walter P. McConaughy, Memorandum of Discussion of Yu Ta-wei, Robertson, *et al.*, December 6, 1955, Offshore Islands 1955, Box 53, Office of Chinese Affairs 1948–1956, RG 59, National Archives. Taiwan's *Central Daily News* dismissed Zhou's Bandung statements as "nonsense" and stated that the aims of his overture were to divert world attention from the mainland's military build-up, to undermine free world unity, and to subvert U.S.-Nationalist relations; April 28, 1955, p. 1.

[61] Han, *Diplomacy of Contemporary China*, 96–98; *FRUS*, 1955–57, 2: 230–31, 266–67.

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comments had not been made as publicly and as dramatically as those in Bandung. Beijing's negotiation signals had been weak, and the Eisenhower administration generally dismissed such statements as ploys or propaganda. Moreover, the administration had been under great pressure by key Republican leaders not to have contact, let alone negotiate, with the Chinese Communists. Even after Zhou's Bandung speech and the apparent high danger of war in the Strait, Chiang's American supporters fiercely opposed bilateral discussions between Beijing and Washington. Despite this domestic opposition, Zhou's conciliatory statement appeared to the Eisenhower administration by late April to be the last possible way off the "horns of the dilemma," as Dulles had put it. The Radford-Robertson mission to Chiang had failed, military tensions were high, European and Asian allies of the United States were deeply fearful of war, and Zhou's comments had won widespread support among Asian nations. The Eisenhower administration, under tremendous domestic and international pressure, finally had to respond positively.[62]

WITH THE MATERIAL NOW AVAILABLE FROM THE CHINESE SIDE, it is possible to advance a number of revised or new historical evaluations of the crisis. Beijing was partially successful in conducting a controlled military campaign to achieve political ends (bringing world attention to the Taiwan issue) and limited territorial objectives (the capture of Yijiangshan, the Dachens, and other Zhejiang offshore islands). The campaign also resulted in direct discussions between China and the United States, something in which Beijing had been interested for some time.

However, it could also be said, as Mao himself acknowledged, that the campaign set back China's interests in certain respects. Mao had been mistaken about U.S.-Nationalist relations and did not anticipate that his campaign would help push ahead the conclusion of the Mutual Defense Treaty or the Formosa Resolution that gave the president unrestricted authority to deploy U.S. forces in the defense of Taiwan. The treaty became a major political link between Washington and Taibei and later was one of the last obstacles to the eventual rapprochement between China and the United States in the 1970s. (The Formosa Resolution also formed a historical precedent for the 1964 Gulf of Tonkin Resolution that handed over war-making powers to President Lyndon Johnson in Vietnam.) Moreover, the Strait campaign contradicted Mao's wish to reduce tensions in the Southeast Asia region and damaged China's prestige with some of its Asian neighbors.

Beijing also did not understand the serious effect its activity and statements would have in the United States; it did not realize how close Washington came to escalating the crisis to widespread military action. Local Chinese military commanders, and some top officials in Beijing, believed that, since the offshore islands and Taiwan were China's own internal matter, the political reaction of the United States should be ignored.

As for the Eisenhower administration, it, too, made mistakes during the crisis.

[62] Barnett, "Chou En-lai at Bandung"; *FRUS*, 1955–57, 2: 522–23, 507–09, 517, 519–20, 526–28.

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It fundamentally misunderstood the political nature of China's "Liberate Taiwan" campaign and thus played into it. It was the U.S. escalation of the crisis that helped bring world attention to the Strait, as well as dangerously exacerbated tensions there, especially in April 1955. But, most important, the Eisenhower administration erred in its belief that its effort at deterring China from assaulting Quemoy and Matsu was effective. The Quemoy-Matsu crisis of 1954–1955 is not an example of successful deterrence, since there was not an immediate, specific threat to these two island groups. (While it could be said that the United States succeeded in a general effort at deterrence of Chinese military activity in the Strait, since the U.S. presence did make Beijing cautious in its plans toward Taiwan island, this is another matter.)[63] In addition, the Eisenhower administration made important concessions to both the Nationalists (namely, the Mutual Defense Treaty) and the Communists (bilateral talks). Its nuclear threats against the mainland not only stiffened Communist resolve, they also helped convince Beijing to launch its own nuclear weapons program. And through it all, the status of Quemoy and Matsu remained contested, ready to draw the United States into other highly charged confrontations with the mainland, as eventually happened in 1958 and in 1962.[64]

In sum, the episode illustrates the danger of mutual isolation. Without a regular means of communication, the United States and China often misread one another's intentions. Ideological assumptions about adversaries played a large role in policy making at critical moments and distorted, or obscured, objective understanding of the situation. If Chiang had accepted the Eisenhower-Dulles plan to blockade the China coast, large-scale hostilities between Chinese and U.S. forces would likely have resulted. It was good fortune and simple luck that Zhou Enlai, having no apparent knowledge of the content of the Radford-Robertson mission to Taiwan, made the conciliatory statements at Bandung, which led directly to defusing tensions in the area.

The episode also illustrates the complexity of policy making on both sides. Western studies of the crisis often examine the difficulties and constraints Eisenhower faced as the leader of a pluralist, democratic government; but Mao, as the leader of an authoritarian and centralized regime, also had to contend with a number of complicating factors, including domestic sentiments, overeager military commanders, faulty intelligence, and differences with other top officials. Overall, his behavior through the crisis, provocative though it was in challenging the Nationalists in the offshore island area, now appears to be more prudent in operational matters than previously believed.

One might argue that, as a result of the crisis, Chiang Kai-shek and the Nationalists turned out to be winners. The Nationalists lost some small bits of

[63] On the difference between "immediate" and "general" deterrence, see Lebow and Stein, *When Does Deterrence Succeed*, 9–17.

[64] John Wilson Lewis and Xue Litai, *China Builds the Bomb* (Stanford, Calif., 1988), 11–46. One could be even more critical of Washington and argue that the U.S. quandary over the islands was actually self-created. As late as 1953, the Nationalists had not paid much attention to the defense of the offshore islands; it had been the United States that had encouraged the Nationalists to improve their defenses, thus accentuating their importance for the Nationalists. McConaughy, Memorandum of Discussion of Yu Ta-wei, Robertson, *et al.*, December 6, 1955.

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territory that they had already recognized were almost indefensible and had devalued for several years; but they gained a mutual defense treaty, the Formosa Resolution, and significant increases in U.S. military aid, including support for the fortification of Quemoy and Matsu. These helped forge an impressive security guarantee for the Nationalists for twenty-five years. Quemoy and Matsu remain in Nationalist hands. In fact, it appears that Chiang was able to use the pressure from the mainland as leverage to extract important concessions from the United States, but, until we have access to archives on Taiwan, we will not be able to evaluate the actual skill or wisdom of the Nationalist policy makers.

Finally, the episode helps us appreciate the contingency of events and unpredictability of international crises. They can develop in ways that are unforeseen and unintended by all the policy makers involved. In the Quemoy-Matsu case, miscalculation by both sides led to unwanted and unanticipated consequences. Evaluation of intelligence through the colored filter of assumptions based on each side's own culture, national experiences, and biases made mistakes inevitable and contributed to the escalation of tensions. The absence of war was due more to good luck than to effective deterrence, either of the nuclear or Chinese "people's war" varieties; indeed, the U.S. and Chinese practices of their particular forms of deterrence and "brinksmanship" brought wide-scale conflict closer than either side realized at the time, and for that matter, closer than either side has known during the forty years since the confrontation.

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China and Western Technology in the Late Eighteenth Century

JOANNA WALEY-COHEN

> We have never valued ingenious articles, nor do we have the slightest need of your country's manufactures.[1]

BY THE LATE EIGHTEENTH CENTURY, THE BALANCE OF EUROPEAN OPINION had tilted against China. Westerners, earlier in the century almost uncritical in their admiration, came to the conclusion that the Chinese seemed unwilling, or unable, to improve on their earlier inventions, such as gunpowder and the compass, which formed part of the foundation for Western development. The famous assertion of Chinese self-sufficiency quoted above, made in 1793 by the Qianlong emperor (r. 1736–1795) in response to Lord Macartney's embassy from King George III, seemed to epitomize Chinese aloofness to the potential offered by Western knowledge.[2]

Europeans specifically equated this apparent lack of interest in what the West

I am most grateful to the following: Jonathan Spence, Nathan Sivin, Paul Kennedy, Geoffrey Parker, Mark Shulman, Daniel Bays, Beatrice Bartlett, Ju Deyuan, Susan Naquin, Peter Perdue, Edward Malatesta, S.J., John E. Wills, Jr., and Marilyn Young, as well as participants in seminars, colloquia, and panel discussions at (in chronological order) the University of Kansas, Lawrence; Harvard University; Columbia University; New York University; Yale University; and the 1992 Association for Asian Studies Conference in Washington, D.C. Thanks also to archivists and library staff at the Archivum Romanum Societatis Jesu, Rome; at the Archives des Jésuites de Paris, Vanves, France; at the Bibliothèque Nationale, the Bibliothèque de L'Institut de France, and at Yale University; and for financial, institutional, and moral support to the History of Christianity in China project and the Henry Luce Foundation; the Columbia Society of Fellows in the Humanities; and the John M. Olin Postdoctoral Fellowship in Military and Strategic History at Yale University.

[1] "Yingshi Maga'erni Laipin An" [The Case of the English Macartney Embassy and Their Gifts], *Chang Gu Cong Bian* [Collected Historical Records] (Beiping, 1930–43), 3: 16–24, at 19b.

[2] There is an immense primary and secondary literature on this topic. Among the most influential early firsthand accounts were Jean-Baptiste Du Halde's *Description géographique, historique, chronologique, politique et physique de l'empire de la Chine et de la Tartarie chinoise*, 4 vols. (Paris, 1735–39; The Hague, 1736; English trans., London, 1738, 1741; German trans., 1748), based on missionary descriptions; George Anson, *A Voyage round the World in the Years MDCCXL, I, II, III, IV* (London, 1748; Paris, 1749), based on personal experience on the South China coast; and John Barrow, *Travels in China, containing Descriptions, Observations and Comparisons Made and Collected in the Course of a Short Residence at the Imperial Palace of Yuen-min-Yuen, and on a Subsequent Journey through the Country from Pekin to Canton*, 2 vols. (London, 1804), based on the author's observations while in China with Macartney's embassy in 1792–93. On the changing Western views of China during the eighteenth century, see, for example, Basil Guy, *The French Image of China before and after Voltaire* (Geneva, 1963); Louis Dermigny, *La Chine et L'Occident: Le commerce à Canton au XVIII^e siècle, 1719–1833*, 4 vols. (Paris, 1964), esp. "Mythe et réalité de la Chine," 1: 11–80.

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had to offer with a lack of interest in science and practical technology, because at that time the West had come to define itself in terms of, and derive a strong sense of superiority from, its undoubted technological power. From such a perspective, it was an easy step to regarding the Chinese as inferior in an overall sense. These views took firm hold as the nineteenth century unfolded and have remained tenacious to this day. Although scholars have recently exploded the myth of China's "opposition" to Western science, it remains widely believed, and, in the case of technology, neither the conviction of the Chinese lack of interest nor the assumptions on which it rested have been subjected to serious inquiry.[3]

Yet the situation in the eighteenth century was far more complex than Qianlong's public declaration suggests. In the preceding decades, he and a number of others in China had displayed considerable interest in all manner of things Western, particularly science and technology. Although this interest was duly recorded by a range of Western observers and made widely available to their European readers, the overwhelming body of opinion disregarded that evidence in favor of the attitudes outlined above.

This essay draws on both Chinese and Western archival and published sources to argue that, for different reasons, the imperial expression of disdain and Western readiness to accept it were primarily prompted by internal political agendas rather than by actual circumstances and that, as a result, the whole tenor of early Sino-Western relations was based on false premises. More accurate evaluation of the genesis and development of Sino-Western mutual perceptions will help correct a misapprehension whose effects are still evident two centuries after the Macartney mission.[4] The essay analyzes Chinese use of Western knowledge and technical skill in the late eighteenth century, focusing principally on the sequence of events in China in the military sphere. This context was central to Western assumptions of superiority because military culture, including the civil-military relationship, has been and continues to be of signal importance to a proper understanding of China. Yet the extraordinary influence of Chinese civilian culture has led to an unfortunate neglect of these issues.

The circumstances that gave rise to the misunderstanding, the background to

[3] For Western views, see Michael Adas, *Machines as the Measure of Men: Science, Technology, and Ideologies of Western Dominance* (Ithaca, N.Y., 1989); Carlo M. Cipolla, *European Culture and Overseas Expansion* (London, 1970); William H. McNeill, *The Pursuit of Power: Technology, Armed Force, and Society since A.D. 1000* (Chicago, 1982); Joel Mokyr, *The Lever of Riches: Technological Creativity and Economic Progress* (New York, 1990); Theodore von Laue, *The World Revolution of Westernization: The Twentieth Century in Global Perspective* (New York, 1987). On Chinese attitudes, see Pasquale M. d'Elia, *Galileo in China: Relations through the Roman College between Galileo and the Jesuit Scientist-Missionaries (1610–1640)*, Rufus Suter and Matthew Sciascia, trans. (Cambridge, Mass., 1960); Huang Yilong, "Tang Ruowang yu Qingchu Xili zhi Zhengtonghua" [Tang Ruowang (Adam Schall) and the Regularization of Western Astronomy in the Early Qing], *Xinbian Zhongguo Kezhi Shi* [Newly Edited History of Chinese Technology], vol. 2 (Taibei, 1990); Nathan Sivin, "Copernicus in China," in *Colloquia Copernicana II: Etudes sur l'audience de la Théorie Héliocentrique* (Warsaw, 1973); Sivin, "On 'China's Opposition to Western Science during Late Ming and Early Ch'ing,'" *Isis*, 56 (1965): 201–05; Wang Ping, *Xifang Lisuanxue zhi Shuru* [The Importation of Western Astronomical Knowledge] (Nangang, Taiwan, 1966); Harriet Zurndorfer, "Comment la science et la technologie se vendaient à la Chine au XVIII[e] siècle," *Etudes chinoises*, 7 (Autumn 1988): 59–90. See also Nathan Sivin, "Science and Medicine in Imperial China: The State of the Field," *Journal of Asian Studies*, 47 (February 1988): 41–90.

[4] See, for example, Alain Peyrefitte, *L'empire immobile, ou, Le choc des mondes: Récit historique* (Paris, 1989; English trans., New York, 1992).

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which is described in detail below, were essentially as follows. In a manner familiar to those who deal today with Chinese foreign relations, Qianlong's public declaration had a double edge and was intended for a multiple audience. For a variety of reasons, he preferred not to admit publicly his interest in and awareness of the potential of foreign technology. His motivation becomes clearer when the episode is placed within the context of late eighteenth-century Chinese politics. The Manchu Qing dynasty (1644–1911) imposed and ultimately maintained its rule over China by military means while at the same time seeking to present to its Chinese subjects and the world at large an image that was both thoroughly Confucian and ethnically even-handed. For the Qianlong emperor, these somewhat contradictory goals meant, among other things, that he made a virtue of his own civilian accomplishments yet simultaneously leaned toward military culture by, for example, promoting the martial traditions of the Manchus; by awarding high civil office (usually the prize of scholars successful in a series of competitive examinations based on classical Chinese texts) to successful generals, almost all of whom were Manchus; by prohibiting the private possession of any weapon; and by jealously guarding access to all information, especially any that smacked of technology, conceivably of use to would-be rebels. No less important, the emperor's aspirations to universal authority combined with acute political factionalism and the gradual onset of a crisis of dynastic self-confidence to render any suggestion of a lack of imperial autonomy wholly out of the question.[5] Thus the famous statement can be understood, on the one hand, as a piece of propaganda directed at a domestic audience with the dual objectives of morale boosting and intimidation.

On the other hand, the statement was, of course, directed at the king of England through his envoy Macartney and, by extension, at any other foreigners who might individually or collectively seek to alter the structure of China's foreign relations to the disadvantage of ultimate Chinese control. In this context, the emperor's disingenuousness conformed to a pattern dating back at least to the early seventeenth century and still discernible today. According to this pattern, the Chinese have consistently sought to absorb Western practical technical skills while remaining inimical to Western ideologies. This dichotomy originated with the attempted use by Christian missionaries of Western scientific and technical expertise as a means of arousing Chinese interest in the Christian religion. Many Chinese, although they fully grasped the utility of the practical knowledge, were hesitant to adopt it because it seemed inseparable from Christianity; accustomed to a political system in which ideology specifically either served orthodox authority or constituted rank heresy, they sensed the subversive potential of the foreign religion.[6] Thus, as we shall see, when rebels threatened the Qing dynasty in the late seventeenth century, the Kangxi emperor (r. 1661–1722) was happy to

[5] On Qianlong's view of his own monarchy, see Pamela Kyle Crossley, "The Rulerships of China," *AHR*, 97 (December 1992): 1468–83; Crossley, "*Manzhou Yuanliu Kao* and the Formalization of the Manchu Heritage," *Journal of Asian Studies*, 46 (November 1987): 761–90; Harold L. Kahn, *Monarchy in the Emperor's Eyes: Image and Reality in the Ch'ien-lung Reign* (Cambridge, Mass., 1971); Beatrice S. Bartlett, *Monarchs and Ministers: The Grand Council in Mid-Ch'ing China, 1723–1820* (Berkeley, Calif., 1991); Philip A. Kuhn, *Soulstealers: The Chinese Sorcery Scare of 1768* (Cambridge, Mass., 1990).

[6] Jacques Gernet, *China and the Christian Impact: A Conflict of Cultures* (Cambridge, 1985).

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improve his arsenal under Jesuit direction, yet he clearly recognized the actual and symbolic threat that papal authority over Chinese Christians would pose and rejected it absolutely. In the eighteenth century, even as Western missionaries' technical advice helped save Qing armies, Christian efforts to proselytize in the provinces were met with persecution. Subsequently, in the late nineteenth century, certain Chinese reformers sought to acquire the Western technology that would bring their nation wealth and power without abandoning the indigenous intellectual tradition. In a twentieth-century variation on this theme, Chinese leaders of the early People's Republic resisted subservience to Soviet control even at the cost of substantial setbacks in technical programs, such as building the atomic bomb, that were intended to bring China into a position of equality with its principal ideological foe, the United States. Most recently, Chinese rulers have continued to be wary of Western ideas, initiating campaigns against "spiritual pollution," "bourgeois liberalization," and "peaceful evolution" (to capitalism), and have endeavored to introduce economic reform in isolation from political reform.

In other words, the Chinese, and their rulers, have uniformly displayed a powerful reluctance to surrender authority or autonomy to any outsider or even to take a chance of doing so. This attitude must be distinguished from the isolationism, the hostility to innovation, especially when of foreign origin, and the immutable sense of superiority for which it has often been mistaken.

Responsibility for the late eighteenth-century miscommunication cannot, however, be laid entirely on the Chinese, for the shift in European views of China, like the Chinese denial of interest in the West, tended to reflect internal, subjective conditions rather than a change in China itself. The eyewitnesses who made note of Chinese interest in the West and what it had to offer included Jesuit missionaries, whose correspondence was published and widely read in Europe at the time, and members of Macartney's mission, who recorded it in their memoirs of the embassy. Unfortunately, these observations came to be superseded in Western minds by the impression, recorded in other such accounts, of the Chinese as sorely deficient in the inquiring and progressive spirit that Europeans considered one of their own culture's most enviable characteristics. There were a number of reasons for this. One was the steady decline of the Society of Jesus, whose members had once held a virtual monopoly on the interpretation of China to Europe. The triumph of those who opposed the Jesuits (the Society was abolished in 1773) seemed to confirm the unreliability of Jesuit accounts. At least as important an influence on changing European views of China was a series of momentous developments in Europe, in particular industrialization and the new focus on political liberty, with all the profound intellectual shifts that accompanied these metamorphoses.

The period of Sino-Western interaction that concerns us began with the arrival in China of Catholic missionaries in the late sixteenth century. Among these, Jesuits were the most conspicuously successful, in part because of their "top down" strategy, which brought them access to the country's elite and to the imperial court. The cartography and more direct military involvement of some of the early luminaries of the Jesuit mission are well known; these are briefly

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recapitulated, however, since they created for both the missionaries and their Chinese hosts significant precedents for the events in the eighteenth century on which the main body of this essay turns.

JESUIT MISSION STRATEGY IN CHINA was controversial for its accommodative approach and for its attempts to arouse the interest of Chinese literati in Western scientific and technical knowledge.[7] Among the most impressive achievements made in the course of these endeavors were those in the realms of cartography and geography, then advancing by major strides in Europe. Thus Matteo Ricci (1552–1610) produced in 1602 a world map that intrigued his Chinese contacts, even attracting the attention of at least one of the late-Ming emperors; in 1674, Ferdinand Verbiest (1623–1688), who built many of the instruments that can still be seen today at the Jesuit Observatory in Beijing, brought out an updated version of the world map that synthesized the knowledge acquired in the intervening decades. Other important early work in this area included the *Novus atlas sinensis* of Martinus Martini (1631–1661), published in 1655.[8]

Some Jesuit missionaries who displayed exceptional talent in these realms sowed the seeds of military assistance to the Chinese. Since classical antiquity, the Chinese had been well aware of the military significance of cartography, for the classic *Art of War* by the great strategist Sun Zi (Sun Tzu) had stressed the importance of "knowing one's terrain."[9] That lesson was not lost on successive rulers of the seventeenth century, the first half of which saw almost continuous warfare as the result of the dynastic transition. The seeds began to bear fruit during the period of dynastic consolidation under the Kangxi emperor.

His reign coincided with the high tide of Jesuit influence in China. Kangxi himself greatly valued Jesuit cartography; under his auspices, missionaries undertook a ten-year survey of the entire empire that formed the foundation for all subsequent geographic study of China—incidentally offering the missionaries

[7] The literature on the Jesuit mission to China is huge. For bibliographic references, see Carlos Sommervogel, *Bibliothèque de la Compagnie de Jésus*, 12 vols. (Brussels and Paris, 1890–1932); Erik Zurcher, Nicolas Standaert, and Adrianus Dudink, *Bibliography of the Jesuit Mission in China: Ca. 1580–ca. 1680* (Leiden, 1991); Robert Streit and Johannes Dindinger, *Bibliotheca missionum* (Rome, 1951–73), vol. 7: *Chinesische Missionsliteratur, 1700–1799*. See also Joseph Dehergne, "Les Archives des Jésuites de Paris et l'histoire des missions aux XVII^{e} et XVIII^{e} siècles," *Euntes docete*, 21 (1968): 191–213; Dehergne, *Répertoire des Jésuites de Chine de 1552 à 1800* (Rome, 1973); Antoine Gaubil, *Correspondance de Pékin, 1722–1759*, Renée Simon, ed. (Geneva, 1970); Louis Pfister, *Notices biographiques et bibliographiques sur les Jésuites de l'ancienne mission de Chine, 1552–1773* (Shanghai, 1932). On Jesuit accommodation, see David E. Mungello, *Curious Land: Jesuit Accommodation and the Origins of Sinology* (1985; rpt. edn., Honolulu, 1989).

[8] Jonathan D. Spence, *The Memory Palace of Matteo Ricci* (New York, 1984), 64–65, 149. The 1602 map was a revised version of one Ricci had made in 1584. On Verbiest, see Minako Debergh, "Une carte oubliée du P. Ferdinand Verbiest," *Journal asiatique*, 277 (1989): 159–219; I am indebted to Franciscus Verellen for drawing my attention to this article. On Martini, see Mungello, *Curious Land*, 116–24.

[9] Sun Zi's dates are uncertain; he probably lived sometime between the fifth and first centuries B.C. In 221 B.C., the conquering first emperor had assembled all available maps, presumably with a view to keeping the information to himself. A chapter of the ancient text *Guanzi* concerns military maps; it is unclear whether this in fact predates the Han dynasty (206 B.C.–220 A.D.). Joseph Needham, *Science and Civilization*, Vol. 3, *Mathematics and the Sciences of the Heavens and the Earth* (Cambridge, 1959), 535–36.

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the opportunity to proselytize in areas they otherwise might never have been able to visit. This survey was later engraved for the Chinese on forty-four copper plates by Abbé Matteo Ripa (1682–1745).[10] Other Chinese-sponsored Jesuit cartography focused on outlying regions, extending far beyond the imperial frontiers toward the borders of Russia and Persia. One of the most notable exponents was Antoine Gaubil (1689–1759), who in addition to drawing maps recorded his observations about the areas in which he traveled, very much in the Chinese tradition of descriptive geography.[11] Although Chinese expertise in cartography was already considerable, the Chinese particularly admired the greater precision of Western techniques for drawing and reproducing maps. Recognizing the Westerners' evident superiority in astronomy, which involved techniques similar to those required for land surveys, they appointed a Jesuit missionary in 1644 to direct the Imperial Board of Astronomy, and Jesuits filled this position for over a century.[12]

The Kangxi emperor was profoundly interested in all manner of Western practical knowledge, employing missionaries to instruct him in Western technology, science, and music. Notably, he sought to advance the dissemination of Western scientific and technical knowledge by his patronage of a number of mathematicians and astronomers and by the promotion of relevant publications, including, for example, a compendium of mathematics (*Shu Li Jing Yun* [Collected Essentials on Numbers and Their Principles]) that included a portion of Euclid translated by Ricci.[13]

One area of Western knowledge that especially interested Kangxi was artillery. Gunpowder, invented by the Chinese in the tenth century, had found its way to Europe three centuries later with the Mongols. Spurred on by the constant warfare between the European states, the use of artillery had developed to a more advanced stage in Europe than in China. Western cannon were relatively light and mobile, although the evidence suggests that the Chinese still retained an advantage in the technology of gunpowder, as distinct from that of weapons construction.[14] In the early sixteenth century, European traders brought their armaments and their iron and bronze casting techniques back across the world to China through Japan, India, and Southeast Asia; by no later than the 1620s, Chinese workers were casting cannon in Macao under the direction of Portuguese

[10] Pfister, *Notices biographiques et bibliographiques*, 608 (biography of Fridelli, one of those who worked on the Kangxi survey): "dans ses immenses voyages . . . il . . . ne manqua jamais l'occasion d'annoncer l'Evangile et de prêcher aux Païens."

[11] Gaubil, *Correspondance de Pékin*, esp. 235–37, letter of October 13, 1729; Henri Bernard, "Les étapes de la cartographie scientifique pour la Chine et les pays voisins depuis le seizième jusqu'à la fin du dix-huitième siècle," *Monumenta serica*, 1 (1935): 428–76, at 466–70; Pfister, *Notices biographiques et bibliographiques*, 666–93.

[12] On the association of astronomy and cartography since antiquity in China, see Needham, *Science and Civilization*, 3: 542; on Europe, see Josef W. Konvitz, *Cartography in France, 1660–1848: Science, Engineering, and Statecraft* (Chicago, 1987), 4. On the significance of astronomy to the Chinese, see Gernet, *China and the Christian Impact*, 61–62.

[13] See Wang, *Xifang Lisuanxue zhi Shuru*, esp. 69–74.

[14] See McNeill, *Pursuit of Power*, 81–89; Needham, *Science and Civilization*, Vol. 5, *Chemistry and Chemical Technology*, part 7, "The Gunpowder Epic" (Cambridge, 1986), 139–41, and 398–407, where Needham argues persuasively for the achievements of Chinese indigenous artillery in the early seventeenth century.

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gun founders, whose work was already in demand throughout colonial Asia.[15] At the suggestion of high-ranking Chinese converts, these Portuguese artillery technicians more than once were invited north from Macao to help the Ming against the Manchus.

Meanwhile, at court, the lucidity of the first incumbent at the Imperial Board of Astronomy, Adam Schall (1592–1666), on the usefulness of artillery for the defense of Beijing led to an order that he direct the casting of cannon for the failing dynasty. His principal contribution was the production of smaller and less unwieldy siege guns. He reduced their size from 75-pounders to 40-pounders, eventually producing over five hundred pieces.[16] Schall also collaborated with a Chinese colleague on a work about gunnery titled *Ze Ke Lu* (Record of Immediate Conquest), also known as *Huo Gong Jie Yao* (Essentials of Gunnery). Originally published in 1643, it was reprinted in 1841 at the height of the Opium War.[17]

Schall was succeeded as director of the Board of Astronomy by Ferdinand Verbiest, whom Kangxi, beset by rebellion, soon pressed into cannon founding as well.[18] Verbiest's task, much as Schall's had been, was to cast lighter and more mobile artillery than the Manchus already possessed, capable of being carried over mountains and rivers. Rather than introducing imported Western types, he improved on Chinese cannon, lengthening the barrels and in some cases using different materials.[19] Over a fifteen-year period, his cannon foundry produced almost five hundred artillery pieces, made of cast iron and bronze, which were used both in the suppression of the Rebellion of the Three Feudatories (1673–1681) and in the Russian campaigns of 1685–1686. The foundry continued in operation after his death in 1688, and the Chinese were still using his designs at the time of the Opium War in 1839.[20] Indeed, the cannon made by Schall and Verbiest remained an important part of the imperial arsenal until the end of the

[15] Charles R. Boxer, "Asian Potentates and European Artillery in the +16th to +18th Centuries: A Footnote to Gibson-Hill," *Journal of the Malayan Branch of the Royal Asiatic Society*, 38, part 2 (December 1965): 156–72, at 169; see also Geoffrey Parker, *The Military Revolution: Military Innovation and the Rise of the West, 1500–1800* (Cambridge, 1988), 136–40.

[16] Whereas earlier Chinese cannon had been comparable to the largest of contemporary European artillery, the size of Schall's cannon corresponded to some of the smallest in Europe, at the same time probably reducing the range from approximately 750 to approximately 400 yards. See R. Ernest Dupuy and Trevor N. Dupuy, *Encyclopedia of Military History from 3500 B.C. to the Present*, 2d rev. edn. (New York, 1986), 453.

[17] Jiao Xu, *Ze Ke Lu*, also known as *Huo Gong Jie Yao* (n.p., 1841); Needham, *Science and Civilization*, vol. 5, part 7, 394; Pfister, *Notices biographiques et bibliographiques*, 165; Jonathan D. Spence, *To Change China: Western Advisers in China, 1620–1960* (1969; rpt. edn., New York, 1980), 15.

[18] For Kangxi's order, see *Daqing Shengzu Shilu* [Veritable Records of the Kangxi Emperor], *juan* 49, 6b, 13/8/11 (September 10, 1674). Initially reluctant to comply, as Schall had been before him, Verbiest yielded only in the face of the emperor's threat to execute him and expel all Christians from China. Yet his insistence that priests were men of peace who knew nothing of the techniques of war was disingenuous. Jesuits were notorious for their predilection for "unpriestly," that is, political, activity. Ignatius Loyola, the founder of the Jesuit order, was a soldier who embarked on a religious career only after being wounded. He originally called the Society a "company" so as to express the soldierly spirit of loyalty to the captain, Jesus.

[19] Louis Le Comte, *Nouveaux mémoires sur l'état présent de la Chine* (Paris, 1696), 2: 246, 248. On Le Comte as an early critic of Chinese science and technology, and his influence, see Adas, *Machines as the Measure of Men*, 81–83.

[20] See Shu Liguang, Hu Jianzhong, and Zhou Cheng, "Nan Huairen yu Zhongguo Qingdai Zhuzao de Dabao" [Ferdinand Verbiest and the Casting of Cannon in China during the Qing], *Gugong Bowuyuan Yuankan* (1989): 28, citing the Imperial Household Archives, Beijing.

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dynasty.[21] In a curious quirk of history, a number of these weapons are now in European museums, having been captured during foreign wars of the nineteenth century.[22]

Verbiest titled his treatise on artillery and ballistics, published around 1681 but now lost, *Shenwei Tushuo* (Explanations and Illustrations of [the Cannon Named] Wonderful and Terrible). In this work, he wrote on the importance of uniformity in the weight of cannonballs and on the critical difference a cannon's angle of elevation could make to its accuracy. If soldiers knew the exact distance between the target and the gun, and the weight of the cannonball, they could accurately hit their targets. The unmistakable implication, and one with important reverberations almost a century later, was that accurate land surveys were just as critical for this purpose as for merely "knowing one's terrain."[23]

Jesuit influence in China gave rise to tremendous hostility on the part of competitors in the mission field and in the highly factionalized European Catholic establishment. Thus it was hardly surprising that Verbiest's "unpriestly" activities as a cannon founder should have prompted virulent condemnation, such as that published by the Dominican priest, Domingo Navarrete (d. 1689) in Madrid in 1676. Yet Verbiest had powerful support. In 1681, Pope Innocent XI praised him publicly for "having used the profane sciences for the safety of the people and the advancement of the Faith."[24] Jesuit cartography was harder to criticize than their gun founding because it helped to disseminate accurate information about China in the West as well as to serve Chinese purposes. For example, the missionary survey of the empire undertaken for the Kangxi emperor was reproduced in Europe, with revisions: in France by the celebrated geographer Jean-Baptiste Bourguignon d'Anville (1697–1782) in the early 1730s and in Holland a few years later.[25] Appearing in 1735 as a supplement to the first comprehensive European account of China—Du Halde's *Description géographique . . . de la Chine*—it intrigued a Europe still enthusiastic about China and hungry for direct information.

Jesuit influence in Beijing declined sharply after a papal mission in 1705–1706 attempted to extend Rome's authority over Chinese Christians. This move served only to confirm the worst Chinese suspicions about the Western religion and resulted in failure. Missionary involvement in the succession dispute that embittered the first decades of the eighteenth century in China further eroded Jesuit

[21] *Qingdai Chouban Yiwu Shimo* [Annals of the Management of Barbarian Affairs during the Qing Dynasty, Tongzhi Reign (1862–1874)] (Beiping, 1930), 100 *juan*, 1863: 35a-b, which indicates that Schall's pieces were still in use at least in the eighteenth century. Thus the suggestion of Arnold H. Rowbotham, *Missionary and Mandarin: The Jesuits at the Court of China* (Berkeley, Calif., 1942), 97, that Verbiest was pressed into service as a cannon founder because Schall's cannon were "either too heavy for field purposes or had fallen into disrepair," was mistaken.

[22] For a table showing the location of Verbiest's cannon in European museums, see Giovanni Stary, "The 'Manchu Cannons' Cast by Ferdinand Verbiest and the Hitherto Unknown Title of His Instructions" (forthcoming), 6. I am indebted to Professor Stary for making this work available to me. For specifications of cannon cast at this time, at least some of which were almost certainly under Verbiest's direction, see also *Daqing Huidian Shili* [Institutes and Precedents of the Great Qing] (1899; rpt. edn., Taibei, 1976), *juan* 894, 1a-5a.

[23] Du Halde, *A Description of the Empire of China* . . . (English edition), vol. 2, 82; Stary, "Manchu Cannons"; Shu, Hu, and Zhou, "Nan Huairen yu Zhongguo Qingdai Zhuzao de Dabao," 30.

[24] See Rowbotham, *Missionary and Mandarin*, 98, 247. The laudatory papal brief appeared in the same year as Verbiest's own defense against Navarrete's attack, *ibid.*, 314.

[25] Bernard, "Les étapes de la cartographie scientifique," 462; Konvitz, *Cartography in France*, 33–35.

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prestige; both the Yongzheng emperor (1723–1735) and the Qianlong emperor banned missionaries altogether from the provinces, although few if any specific steps were taken to eradicate Christianity in China. In Europe, the failure of the papal mission also hastened the Jesuits' decline because many Catholics, feeling much the same as did the Chinese, feared the consequences of surrendering church control over Chinese converts.

By the 1770s, profound disagreement among Jesuits in Beijing regarding the appropriate response to the papal brief that suppressed their order around the world weakened the mission's already unpopular cause in the eyes of the Chinese, who nonetheless maintained their strong interest in missionaries' scientific knowledge and technical skills.[26] Under Chinese auspices and at their behest, European missionaries at the Qianlong court carried out a range of technical activities themselves and trained Chinese in many of the necessary skills. They designed an imperial palace near Beijing and installed European-style fountains there; they made elaborate clocks and mechanical toys; they provided technical advice on glassmaking and supervised its production, constructing furnaces of their own design; they built complicated hydraulic and other machinery, for the operation and function of which they provided detailed explanations in response to Chinese requests; and they assembled devices for applying electroconvulsive shock treatment for nervous illness, administering it with some success. These activities met with an enthusiastic response from the emperor, imperial family members, and government officials. Missionaries also helped further Qianlong's military ambitions by offering instruction on the use and construction of firearms and by mapping newly conquered areas; and they made copper engravings of maps and pictures of Chinese military victories or arranged to have such engravings made for the Chinese in France until they could train Chinese to do the job.[27] With one exception—advice on artillery—none of these activities was in any sense covert.

[26] See Joseph Krahl, *China Missions in Crisis: Bishop Laimbeckhoven and His Times, 1738–1787* (Rome, 1964), 223 and following; see also Bernward Willeke, *Imperial Government and Catholic Missions in China during the Years 1784–1785* (St. Bonaventure, N.Y., 1948), 14–15.

[27] In Western sources, a large part of the evidence for Chinese interest in Western science and technology can be found in Jesuit correspondence from Beijing, circulated at the time and now located in the Bibliothèque de l'Institut de France, Paris (hereafter, Bib. Inst.), ms. vols. 1515–24; Archivum Romanum Societatis Iesu, Rome (hereafter, ARSI), Fondo Gesuitico and Jap. Sin. collections; Henri Cordier, "Les correspondants de Bertin, sécretaire d'état au XVIII^{e} siècle," *T'oung Pao*, 2^{e} série, 14 (1913): 227–57, 465–72, 497–536; *T'oung Pao*, 2^{e} série, 18 (1917): 295–379; *Lettres édifiantes et curieuses écrites des missions étrangères*, 14 vols. (Lyon, 1819, orig. pub. in Paris, 1702–76, and subsequently rearranged) (hereafter, *LEC*), vol. 13. For some of the extensive secondary literature, see, in addition to the works by d'Elia, Sivin, Wang, and Zurndorfer cited in note 3, and specific references cited in the remaining notes below, P. Huard and M. Wong, "Les enquêtes françaises sur la science et la technologie chinoises au XVIII^{e} siècle," *Bulletin de l'Ecole Française d'Extrême-orient*, 53 (1966): 137–226; Cécile Beurdeley and Michel Beurdeley, *Giuseppe Castiglione: A Jesuit Painter at the Court of the Chinese Emperor*, Michael Bullock, trans. (London, 1972); Cheryl Ann Semans, "Mapping the Unknown: Jesuit Cartography in China, 1583–1773" (Ph.D. dissertation, Geography, University of California, Berkeley, 1987); Nathan Sivin, "Wang Hsi-shan," *Dictionary of Scientific Biography*, 15 vols. (New York, 1970–78), 14: 159–68; Yang Boda, "A Brief Account of Qing Dynasty Glass," in Claudia Brown and Donald Rabiner, *The Robert H. Clague Collection: Chinese Glass of the Qing Dynasty, 1644–1911* (Phoenix, Ariz., 1987), 71–86.

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BOTH EUROPEAN AND CHINESE SOURCES indicate that Qianlong personally demonstrated a clear interest in Western science and technology only a few years before the rebuff to King George. In a series of conversations held in 1773, Qianlong quizzed the missionary Michel Benoist (1715–1774) about Western science, philosophy, warfare, cartography, shipping, and navigational practices.[28] During the 1770s and early 1780s, as the suppression of the order caused the flow of Jesuit missionaries to dwindle, the emperor on several occasions reiterated his desire that more missionaries be sent to serve at the imperial court.[29] Even in 1793, the emperor's response to his British visitors belied the assumption of his indifference, for he had them demonstrate several of the instruments with which they presented him; the single gift that most intrigued him was a model of the 110-gun warship *Royal Sovereign*, about which he asked an impressive number of technical questions. His response echoed the preoccupation with artillery that the emperor displayed in correspondence with his generals.[30]

The West and its scientific knowledge did not excite the interest of the Qianlong emperor alone. It also interested a number of leading scholars of the day, particularly those associated with the evidential research (*kaozheng*) movement, then a major force in intellectual life. Influential scholars such as Dai Zhen (1724–1777), Qian Daxin (1728–1804), and Ruan Yuan (1764–1849) recognized the relevance of Western scientific methods to the *kaozheng* quest for precision and accuracy in all aspects of scholarly endeavor. These and other scholars sought to incorporate Western knowledge on mathematics and astronomy and renewed their interest in the indigenous Chinese scientific and technical tradition. One example of this trend was a 1799 publication, *Chouren Zhuan* (Biographies of Astronomers and Mathematicians), compiled by Ruan in collaboration with Qian and others, which among other things summarized the works of almost three hundred mathematicians and astronomers, thirty-seven of whom were Westerners.[31]

The attitudes of Chinese scholars toward foreign scholarship varied. Some were reluctant to acknowledge the source of the Western scientific information they found so compelling, but they did not on that account reject it. Instead, they sought legitimation for their studies by asserting that Western knowledge had

[28] See, for instance, undated letter of Michel Benoist, in *LEC*, 13: 427–45. It was in this conversation that the emperor asked Benoist whether "your Western philosophers have solved a problem that has much exercised our philosophers here: which came first, the chicken or the egg?" *ibid.*, 441. On Qianlong's curiosity about the West, see also Benoist's letter of November 16, 1773, reproduced in *T'oung Pao*, 2[e] série, 18 (1917): 341–49; George Staunton, *An Authentic Account of an Embassy from the King of Great Britain to the Emperor of China*, 3 vols. (London, 1797), 3: 141.

[29] See, for example, *Daqing Gaozong Shilu* [Veritable Records of the Qianlong Emperor of the Great Qing] (Tokyo, 1937–38), *juan* 1066, 21a-b, 43/9/8; *juan* 1130, 9b-10a, 46/5/3. (Chinese dates are cited as follows: year of reign, lunar month, day; a *juan* is comparable to a chapter.) The emperor called a halt to the visits of missionaries in 1785 because he had enough Westerners working for him. See *juan* 1240, 14a-b, 50/10/8.

[30] Staunton, *Authentic Account*, 3: 140–41. See also John Cranmer-Byng and Trevor Levere, "A Case Study in Cultural Collisions: Scientific Apparatus in the Macartney Embassy to China," *Annals of Science*, 38 (1981): 503–25. On Qianlong's preoccupation with artillery, see, for example, Nayancheng, *A Wencheng Gong Nianpu* [Chronological Biography of Master A Wencheng (Agui)], 8 vols. (n.p., 1813), *juan* 15–16.

[31] Ruan Yuan, Ling Tingkan, Qian Daxin, and Jiao Xun, eds., *Chouren Zhuan* [Biographies of Mathematicians and Astronomers] (n.p., 1799). On this text, see Zurndorfer, "Comment la science et la technologie se vendaient à la Chine au XVIII[e] siècle," 75–76; Yoshio Mikami, "The Ch'ou-jen Chuan of Yuan Yuan," *Isis*, 11 (January 1928): 123–26.

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been transmitted from China, or they criticized the Confucian tradition for preferring metaphysics to mathematics. Some of the information emanating from the West seemed suspect to Chinese intellectuals because of its inconsistencies. Earlier church restrictions on the missionaries had prevented them from revealing the full extent and import of the discoveries by men such as Copernicus and Galileo. When, in the mid-eighteenth century, the uncensored version of Copernican theory finally reached China, Chinese intellectuals were cautious about embracing it precisely because it seemed to contradict the earlier version. The Western attribution of this skepticism to an ingrained Chinese hostility to foreign ideas thus failed to reflect the true sequence of events. Indeed, one may question whether the incomplete way in which some of the new knowledge was transmitted to China may not actually have interfered with scientific progress in China.[32]

Members of the eighteenth-century elite were also cautious about showing too much interest in Western military technology but for different reasons relating to the complicated quagmire of Manchu-Chinese relations. Some senior Manchus, notably those involved in conducting a series of contained military campaigns, were understandably interested in any technology that might bring them victory. For many such men, military success was a surer path to political power than the civil service examinations in which they would have faced fierce competition from Chinese scholars.[33] This situation was complicated by the Qing policy of being and appearing to be legitimately Confucian rulers, which included downplaying military achievement despite the access to political power it often provided.

For others curious about Western military technology, the issues were somewhat different, because Manchu sensitivities to slurs against their alien origin and hence their legitimacy inhibited the open expression by scholars, most of whom were Chinese, of any such interest. Thus the private dissemination of military information, such as the technology detailed in earlier military compendia and anything susceptible of interpretation as anti-dynastic or in general "prejudicial to national security," was banned; any breach was subject to severe punishment. Although this prohibition undoubtedly exercised a constraining influence, a scattering of relevant literary material indicates it failed to suppress such interest altogether. Most such literature dates from the last decade of the eighteenth century (after the Macartney mission) or the early nineteenth century; by this time, censorship had relaxed somewhat, and unstable conditions within China were beginning to bring military information to the forefront of scholarly attention. For example, in the 1790s, the well-connected scholar Sun Xingyan (1753–1818) produced a new annotated edition of Sun Zi (*Sun Zi Shi Jia Zhu* [Sun Zi with Commentaries]), and an official who had been besieged by rebels in Guizhou province produced a detailed manual on urban defense based on personal experience (*Wu Pei Ji Yao* [Collected Essentials of Military Prepared-

[32] See Sivin, "Copernicus in China"; Wang, *Xifang Lisuanxue zhi Shuru*, 80–88; Benjamin A. Elman, *From Philosophy to Philology: Intellectual and Social Aspects of Change in Late Imperial China* (Cambridge, Mass., 1984), 62–64, 80–83; Annping Chin and Mansfield Freeman, *Tai Chen on Mencius: Explorations in Words and Meaning; A Translation of the Meng Tzu tzu-i shu-cheng* (New Haven, Conn., 1990), 8, 179 n. 36; Zurndorfer, "Comment la science et la technologie se vendaient à la Chine au XVIII[e] siècle," esp. 75–76.

[33] See Bartlett, *Monarchs and Ministers*.

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ness]). In 1833, Zhao Xuemin's work on fireworks (*Huo Xi Lüe* [Treatise on Fireworks]), written and probably privately circulated in the mid-eighteenth century, was published; it included detailed information on the composition of gunpowder that could in theory have been adapted for subversive purposes by enemies of the state.[34]

Military matters played a salient role in Western, especially Jesuit, interpretations of China during the second half of the eighteenth century and in missionary activities on behalf of the Qing. In this respect, one important figure was Jean-Joseph Marie Amiot, who lived in Beijing from 1751 to 1793. Amiot maintained a voluminous correspondence with the French minister and sometime comptroller-general, Henri Bertin (1720–1792), then one of the China mission's principal supporters in France and a man whose own fascination with military affairs encouraged Amiot to develop a strong interest in and knowledge of Chinese military affairs. Amiot's letters to Bertin repeatedly addressed such topics as the structure of the Chinese army; the varying composition of gunpowder when used for different purposes—on more than one occasion, he sent Bertin samples for chemical analysis; the metalworking techniques for constructing a type of gong used for military communications and to keep troops in order; and the nature of Chinese military strategy, several classical texts on which he translated into French.[35] Amiot also described in considerable detail the course of various Chinese military campaigns. Although his accounts may be inaccurate in certain points of detail, overall they are corroborated by Chinese sources; they probably reflect information then current at court, obtained from persons in whose access to knowledge and the corridors of power he expressed great confidence.[36] These accounts seem to have been intended to show the Qing armies in a favorable light, but on at least one occasion Amiot infuriated an anonymous European friend of Bertin's, by justifying what the friend considered to be the completely dishonorable and barbaric Qing treatment of captured rebels.[37]

[34] Sun Xingyan, *Sun Zi Shi Jia Zhu* (n.p., 1855, orig. date of publication not given); Xu Xuefan, "Wu Bei Ji Yao," in Xu Naizhao, comp., *Min Guo Zhai Qi Zhong* (Guangzhou, 1832). On Zhao Xuemin, see Needham, *Science and Civilization*, vol. 5, part 7, 139–41. On scholarly communication during the late eighteenth century, see Elman, *From Philosophy to Philology*, esp. 172–229.

[35] For Amiot's partial description of Chinese military organization, which he took from "a military almanac published four times a year," see Amiot to Bertin, July 28, 1777, Bib. Inst. 1515, 119; for his discussions of the composition of gunpowder (which suggest he grasped the greater sophistication of Chinese knowledge), see his letters to Bertin of July 15 and November 5, 1778, Bib. Inst. 1515, 214, 220. For his translation of classical texts on strategy, including Sun Zi's *Art of War*, see his "Art militaire de la Chine," in *Mémoires concernant l'histoire, les sciences, les arts, les moeurs, les usages, &c. des Chinois: Par les missionaires de Pékin* (Paris, 1777–1814), vol. 7; "Supplément," vol. 8. Amiot's "Art militaire" had already been published separately in Paris by Didot in 1772; it was accompanied by dozens of sketches of Chinese crossbows and cannon, siege weapons, armor, and warships. Despite its flaws, the translation of *Art of War* became a classic in its own right in France. For an assessment of Amiot's translation and its reception, see Sun Tzu, *The Art of War*, Samuel B. Griffith, trans. (Oxford, 1971), 179–82.

[36] See October 5, 1771 (on the Burma campaign), Bib. Inst. 1515, 10; September 12, 1776 (on the Second Jinchuan War), Bib. Inst. 1515, 73 and following; August 17, 1781 (on the Hezhou Muslim uprising), Bib. Inst. 1516, 276–77; July 1, 1788 (on the Taiwan campaign), Bib. Inst. 1517, 35. Some of these accounts were published in various editions of the *LEC* or in the 17-volume *Mémoires* cited in note 35.

[37] Bertin to Amiot, November 30, 1777, Bib. Inst. 1522, 154: "le général est *un perfide et son maître un cannibale*" (emphasis in original).

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Amiot also attempted to give the Chinese helpful hints of a military nature. Recognizing the military potential of hot air balloons, then causing a sensation in Europe, he struggled to arouse Chinese interest in them but failed (despite some princely interest) because of the great expense involved.[38] A prince's assurance that "only in war do we have no regard for expense, difficulty or danger; we are ready to try anything" was borne out by other late eighteenth-century Jesuit correspondence.[39] For example, in 1772, noting that the emperor believed that the missionaries in his service knew "something of everything," one of their number reported that the emperor sometimes ordered him and his colleagues to assist at artillery practice or to explain how to use the European arms sent north by Chinese officials based in Canton. Recently chastised by his superiors in Rome for "teaching the infidel the art of war" by developing a plan for the fortification of Beijing, this correspondent also asserted that he and his colleagues had responded to imperial inquiries by denying all knowledge of the techniques required for casting artillery.[40]

The latter assertion was, however, less than forthright, for soon afterward a Jesuit missionary went to the battlefront of a tenacious rebellion specifically to advise on casting and using artillery. During the Second Jinchuan War (1771–1776), native tribespeople in western Sichuan ensconced in a mountainous area had taken refuge in high stone fortresses (*diao*) that the Qing were unable to destroy without bombardment.[41] The precipitous terrain forced soldiers to move in single file and on foot; there was no question of transporting heavy artillery. To some extent, this problem was resolved by the expedient of carrying thousands of metal ingots that could be forged into cannon when and where needed by artisans attached to the army, a solution adopted elsewhere and in earlier times by Turks

[38] On Amiot's enthusiasm for the "globe aërostatique," see his letter of October 20, 1784 addressed to supercargo Roze at Canton; clearly, Roze had sent Amiot details that whetted the latter's appetite for this new mode of transportation. Archives des Jésuites de Paris, Fonds Vivier, vol. 1, 5. "The first use that I will make of it is to go to Canton to see more Frenchmen than I have seen in all the thirty-three years that I have been here." On his failure to interest the Chinese, see Amiot to Bertin, October 10, 1789, Bib. Inst. 1517, 85; see also Bertin to Amiot, December 20, 1787, Bib. Inst. 1524. There was some disagreement at court over the advisability of informing the emperor about hot air balloons: Cordier, "Les correspondants de Bertin," 245–56, letter from the Lazarist Father Nicolas Raux, November 27, 1786. A few years later, a member of Macartney's embassy actually went up in a balloon while in Beijing, but even this seemed not to interest the Chinese. J. L. Cranmer-Byng, ed., *An Embassy to China: Being the Journal Kept by Lord Macartney during His Embassy to the Emperor Ch'ien-lung, 1793–1794* (London, 1962), 54.

[39] Letter to Bertin, July 1, 1788, Bib. Inst. 1517, 52. The prince was "a grandson of Kangxi" but cannot at present be identified with any certainty.

[40] ARSI, Jap. Sin., vol. 184, fols. 261–62; Jean-Mathieu de Ventavon [1735–87] to Father Imbert, S.J., Rome, November 4, 1772. See also Pfister, *Notices biographiques et bibliographiques*, 914, citing an unpublished letter of François Bourgeois [1723–92], October 30, 1769, located in the Xujiawei library near Shanghai. On papal prohibitions on transmitting "the art of war" to Muslims and other "infidels," see Boxer, "Asian Potentates and European Artillery," esp. 160–61.

[41] The main Chinese accounts of the war are *Qinding Pingding Liang Jinchuan Fanglüe* [Imperially Authorized Account of the Suppression of the Two Jinchuan], Wang Chang, *et al.*, comp., 7 vols. (1800; rpt. edn., Beijing, 1985); Nayancheng, *A Wencheng Gong Nianpu, juan* 6–17; Wei Yuan, *Sheng Wu Ji* [Record of Sacred Military (Achievements)] (1842; rpt. edn., Beijing, 1984), *juan* 4; Zhao Yi, *Huangchao Wugong Jisheng* [Record of the Military Achievements of This August Dynasty] (n.p., n.d.; rpt. edn., 1877); see also Erich Haenisch, "Die Eroberung des Goldstromlandes in Ost-Tibet," *Asia Major*, 10 (1935): 262–313.

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and Persians confronting similar logistical difficulties.[42] But these cannon tended to explode after two or three days of heavy firing, which, obviously, limited their usefulness as well as often killing or wounding artillerymen. Moreover, it was virtually impossible to keep the army at the front provided with sufficient raw metal, despite efforts to melt down and reuse exploded cannon barrels.[43] Further problems were caused by the Qing forces' unfamiliarity with the siege methods needed. The Chinese had for centuries been fortifying entire towns with massive walls and were quite familiar with the concept of siege warfare, but the traditional means of ending a siege were mining, mass assault, or blockade, none of which was feasible in this case because of the mountainous terrain.[44] Furthermore, Chinese city walls generally were made of tamped earth rather than of the stone that confronted the Qing armies in western Sichuan.

In 1774, in response to these difficulties, the emperor ordered the Portuguese Jesuit Felix da Rocha (1731–1781) to proceed to the front, bearing with him designs for mortars (*chongtianpao*), or "guns for attacking heaven" (popularly known as "watermelon guns" [*xiguapao*] because of their bulbous shape). The emperor made no pretense: the purpose was to increase the accuracy of his army's artillery.[45] Most likely, the designs in question were those made by Verbiest a century earlier, for these had been republished only a few years before, with illustrations of individual weapons accompanied by specifications and by reproductions of the inscriptions, including the names of artisans and supervisors involved in the actual work of cannon founding.[46]

Although the Chinese sources state unambiguously the purpose for which da Rocha was sent to the front in 1774, the same cannot be said for the Jesuit descriptions of the Jinchuan campaign, written for European consumption. If

[42] Amiot to Bertin, September 12, 1776, Bib. Inst. 1515, 73. See also *Mémoires . . . des Chinois*, 3: 387–412. See Cipolla, *European Expansion*, on the construction of cannon outside the walls of Constantinople; and Robert D. McChesney, "An Untitled History of the War for Khurasan, 1587–1598" (New York, n.d.), 38, on similar activities outside Herat in the late sixteenth century.

[43] On the difficulty of maintaining the supply of raw materials and artisans for artillery production at this time, see the secret reports submitted to the emperor by the governor-general of Sichuan in *Gongzhongdang Qianlongchao Zouzhe* [Secret Palace Memorials of the Qianlong Reign] (Taibei, 1982–92), vol. 36, 69, 39/7/13 (August 19, 1774); vol. 36, 651, 39/9/12 (October 16, 1774); vol. 37, 21–22, 39/9/26 (October 30, 1774).

[44] See Parker, *Military Revolution*, 143–44.

[45] More than one edict expressed the emperor's intention; see Nayancheng, *A Wencheng Gong Nianpu*, 12, 22b-24a, n.d. This document does not refer specifically to Westerners or to any individual missionary, and it may be that the decision to send a Western "surveyor" was made only later; *Daqing Gaozong Shilu*, *juan* 962, 16a-b, 39/7/9; also in Fu Lo-shu, trans., *A Documentary Chronicle of Sino-Western Relations*, 2 vols. (Tucson, Ariz., 1966), 273–74. For an illustration of a *chongtianpao*, see *Huangchao Liqi Tushi* [Illustrations and Plans of Ceremonial Instruments of Our August Dynasty] (n.p., n.d., preface, 1766), 16, 14a; see also Needham, *Science and Civilization*, vol. 5, part 7, 475. This type of mortar apparently was among the gifts made by the British to the emperor in 1793; Fu, *Documentary Chronicle*, 561, n. 468.

[46] For examples of Verbiest's designs, see *Huangchao Liqi Tushi*, 9a-b, 16a-b. This text was compiled under the auspices of Fulong'an (1743–84), the senior official at court who had particular responsibility for the Jesuits. Among those who had worked on the project were He Guozong (d. 1766), a senior mathematician closely associated with a number of Jesuits at court, including da Rocha, who had participated in He's survey of Xinjiang in the 1750s (see below), and a Manchu named Debao, who may have been the same man who accompanied da Rocha to the Jinchuan front or may have been an eponymous and much more distinguished civil official who lived from 1719 to 1789. On the cannon designs, see Shu, Hu, and Zhou, "Nan Huairen yu Zhongguo Qingdai zhuzao de dabao," 28, citing the Imperial Household Archives.

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they mention da Rocha at all, they say only that the emperor sent him to the front that year "pour lever la carte"—to take measurements for a general survey of the area. This claim was plausible since da Rocha had, with a colleague, earlier surveyed the newly conquered region of Xinjiang for the emperor, traveling there even before that war was over.[47] The maps they had produced were kept in the palace; so far as can be ascertained from Chinese and European sources, neither missionary played any other role in the conquest of Xinjiang.[48]

The Chinese and Jesuit sources also differ in their versions of what happened when da Rocha reached the front in October 1774. The Jesuits claimed that da Rocha was so exhausted and unwell that he returned to Beijing without accomplishing his mission.[49] The correspondence between the emperor and commander-in-chief Agui tells a different story. Agui was one of the most powerful men of his time, a Manchu of sufficient scholarly attainment to pass through at least some levels of the civil service examinations. By 1773, he had already held senior military and civilian positions all over the empire; first appointed in 1763 to the Grand Council, the emperor's closest advisory group, he was its senior member from 1780 until his death in 1797 and hence was for almost two decades the ranking official in the entire empire. In his 1774 reports to Qianlong, Agui noted that da Rocha and his escort, a captain of the imperial bodyguard named Debao, had announced that the emperor had sent them because earlier calculations concerning the mortars had been inexact. After da Rocha made various measurements, relating mainly to the angle of firing (just as Verbiest had discussed in his treatise), the margin of error was sharply narrowed, and bombardment of the rebel fortresses became considerably more effective; it was a decisive factor in the eventual Qing victory. Moreover, shortly after da Rocha's arrival, new artillery began to be cast, based presumably on the designs he brought with him and possibly produced under his actual supervision.[50] In short, the Chinese sources reveal that da Rocha was indeed directly involved in "teaching the infidel the art of war."

[47] On Qianlong's personal involvement in the selection of da Rocha for this assignment, see ARSI, Fondo Gesuitico, 722, 3d folder, item 7 (*littera annua* 1756).

[48] ARSI, Jap. Sin., 98, pp. 46 (n.d. [1756–57]); 56 (n.d. [1758]); 62b-63a (November 28, 1758). Some missionaries criticized da Rocha in this context: see Jap. Sin., 98, p. 52, February 7, 1758; see also Jap. Sin., 181, 252–53, December 2, 1752, cited by Krahl, *China Missions in Crisis*, 68. For da Rocha's survey work in Manchuria, see *LEC*, 13: 439 (1773). See also Bernard, "Les étapes de la cartographie scientifique," 472, citing J. Brucker, "Positions géographiques déterminées par les missionaires jésuites dans le Turkestan oriental et la Dzoungarie en 1756 d'après deux lettres inédites des PP. Amiot et Gaubil," *Bulletin de la Société de Géographie de Lyon* (November 1880), published separately, 7–8; Semans, "Mapping the Unknown." On the Xinjiang campaigns, see *Qinding Pingding Zhunke'er Fanglüe* [Imperially Authorized Account of the Suppression of the Zunghars], Fuheng, comp. (n.p., 1772).

[49] *LEC*, 13: 491–504, at 497, letter, n.d. [1777]; this document seems to be much the same as Bourgeois' account of the campaign, "Relation de la Révolte du Pays des Miao-tse," *Mémoires . . . des Chinois*, 3: 412–22, in which the same information about da Rocha appears at 415–16. See also Amiot's long letter to Bertin about the Jinchuan campaign, September 12, 1776, Bib. Inst. 1515, 73 and following, also in *Mémoires . . . des Chinois*, 3: 387–412, and its unattributed supplement, *Mémoires . . . des Chinois*, 3: 412–22.

[50] Nayancheng, *A Wencheng Gong Nianpu* 12, 18a. The dates of these documents are somewhat confused. According to an undated memorial, Debao and da Rocha arrived on the 18th day of the 9th month (October 22, 1774) and proceeded to a test firing of the old cannon on the same day. The document reporting the casting of a new cannon follows this account in the text but is dated the 15th.

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The Jesuit authors of the accounts of the war must have known of da Rocha's direct involvement in improving artillery effectiveness; we can only speculate about their motives in glossing over it. Doubtless aware of the criticisms leveled against Verbiest for his cannon foundry a century earlier, and of the more recent strictures against offering military advice, they may well have tacitly agreed to avoid jeopardizing their already precarious position by revealing what da Rocha had done. Perhaps the need for solidarity in adversity outweighed the antagonisms among the missionaries. This lack of candor, however, inevitably raises questions about the reliability of all Jesuit sources transmitted from China during this period.

Da Rocha visited the Jinchuan region again in 1777 to survey the newly pacified area for his Chinese masters. This time, it seems that he accomplished the task and also took advantage of the prestige with which he was invested by the imperial commission to secure the release of a colleague who for years had languished in a Sichuan prison.[51] His map of the Jinchuan region, said at the time to have been made for the official campaign history (*Qinding Pingding Liang Jinchuan Fanglüe*), was not included in that official text; archivists at the First Historical Archives in Beijing now report that it is located in their collection.[52]

The sequestering of this and other maps such as the Xinjiang surveys brings up the issue of how widely acknowledged da Rocha's military work was in China. Although it may not have been actually concealed, it is also unlikely to have been actively broadcast; as Qing confidence ebbed during the last quarter of the eighteenth century, in considerable part as the result of a dawning recognition of some of the consequences of extraordinary population growth, the reluctance to acknowledge any degree of dependence on foreign advice and assistance undoubtedly intensified. We have only one source, Agui's report, for da Rocha's assistance with artillery; the emperor's expressions of intent to obtain such aid do not establish that da Rocha provided it. Quite apart from Jesuit discretion on the subject in their published correspondence, none of the official Chinese accounts of the war, such as the campaign history, mentions da Rocha's presence at the front, nor do privately published descriptions of the war, written by such well-known scholars as Zhao Yi (1727–1814) and Wei Yuan (1794–1856), contain any reference to Western aid.[53] Given the relatively primitive state of unofficial communications in China at this time, it is probably safe to assume that the Jesuit's advice and assistance was not general knowledge, although the pervasiveness of gossip and rumor should not be underestimated.

[51] *LEC*, 13: 521–25.

[52] See *Gongzhongdang Qianlongchao Zouzhe*, vol. 38, 188, QL 42/3/24, a memorial noting that da Rocha ("Fu Zuolin") had gone to the front to survey the area for the official history (*fanglüe*). Information on the probable present whereabouts and possible non-Chinese origins of da Rocha's Jinchuan map was communicated to the author in person by Ju Deyuan, formerly head of research at the First National Archives in Beijing and a specialist on Jesuits in eighteenth-century China; maps of sensitive areas, such as the Tibetan borderlands here in question, are frequently unavailable to foreign researchers in China. An undated and unattributed map of part of the Jinchuan region that is now in the Bibliothèque Nationale is thought by some scholars to relate to the campaign that ended in 1776. The authorship appears Chinese, although possibly the map represents a copy or outgrowth of da Rocha's work. This map was purchased by the Bibliothèque Nationale from the Klaproth collection.

[53] See references to Chinese sources cited in note 41.

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THIS CONTROL OVER THE FLOW OF INFORMATION made it possible for the emperor to issue his famous statement. Both domestically and internationally, there was considerable propaganda value to be gained from declaring China's self-sufficiency to a foreign state whose potential menace against Chinese national security prompted the Qianlong emperor to warn his officials in Canton about England's naval and mercantile strength. This emperor, with his pretensions to universal monarchy, was hardly likely to admit openly to the representative of a foreign ruler an interest that, in Chinese minds, could be unfavorably interpreted as an intimation of inferiority. The imperial declaration may well also have been subtly intended to remind Qianlong's Chinese subjects that their Manchu rulers remained faithful to the traditional public Chinese attitude of superiority toward foreigners. Although members of the Macartney embassy disparaged what seemed to them to be Chinese disdain for the gifts they brought, those whom they encountered may well have simply been taking their lead from the emperor.[54]

There were other reasons for the ostensible Chinese indifference to the official gifts of their Western visitors, an attitude in apparent conflict with their enthusiasm for what missionaries could provide. First, while it was clear to the Chinese that the missionaries, men of different nationalities divided by internal disputes, were not acting as representatives of their countries (despite efforts on the part of Amiot and the French to assert their close relationship to the French throne), the Macartney embassy unmistakably represented a single foreign ruler. The emperor and his officials understood this distinction, even though their unfamiliarity with Western diplomatic practices probably prevented their grasping the full implications of the embassy.[55] Second, it was obvious that the effective control exercised over Europeans serving at the Qing court could not be duplicated in the case of the Macartney embassy. Thus, whatever their true beliefs, an appearance of condescension was preferable to a display of curiosity.

Third, the involvement of Agui in the 1774 episode drew the entire issue into the intensely factionalized world of late eighteenth-century Chinese politics. Agui participated in a number of the major military campaigns of the middle and later eighteenth century, but it was the Jinchuan war, in which da Rocha's advice had rescued Agui's faltering campaign, that made his reputation; Agui soon became a central figure in one major political faction. Much admired by the Jesuits, he was said to be deeply intrigued by Western knowledge, and it is reasonable to attribute his interest at least partly to his wartime experience. The other main faction at court clustered around Heshen (1750–1795), the imperial favorite and the man in charge of embassy liaison in 1793. Heshen had little experience and absolutely no aptitude for military affairs: on Heshen's only, disastrous, military venture, Agui, arriving at the crucial moment, saved his life and subsequently put in a good word for him with the emperor. Heshen offset the potential political disadvantage of

[54] See Cranmer-Byng, *Embassy to China*, 54.

[55] See James Hevia, "A Multitude of Lords: Qing Court Ritual and the Macartney Embassy of 1793," *Late Imperial China*, 10 (December 1990): 72–105. The fact that, nearly half a century later, the Chinese, almost fatally, failed to realize the importance of such a distinction after the abolition of the East India Company monopoly in 1834 cannot be taken as evidence of a similar failure in 1793. On Qianlong's interest in the different European states and their rulers, see *LEC*, 13: 425–45, n.d.

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lacking military qualifications, however, by his close association with one of the most successful of late eighteenth-century generals, Fukang'an (d. 1796). In 1793, Fukang'an had recently returned from Tibet to take up the governor-generalship of Guangdong and Guangxi provinces. That is, he was in the probably unique position of having encountered the British in both places—as unruly traders on the southeast coast and, as he mistakenly suspected, allies of his enemies, the Gurkhas, in Tibet. Thus Fukang'an's experience led him to regard the British as troublemakers. As a result, he was not only extremely and overtly hostile toward them but also actively sought to dissuade Heshen from helping them or promoting their interests in any way.[56] One can speculate that Heshen and his associates, having been persuaded of the undesirability of the British by Fukang'an, sought to convince the emperor to reject the embassy, hoping at the same time to undermine any advantage that might have accrued to Agui and his adherents as the result of Agui's contacts with the missionaries. Although Macartney himself noted how vigorous the eighty-two-year-old emperor was, Heshen's influence may well have played a part in the entire episode.

Europeans were probably aware of Chinese interest in Western technical skills in yet another military context. The Qianlong emperor regarded the military achievements of his reign as one of its crowning glories, and, after the annexation of Xinjiang, he rebuilt a hall for the specific purpose of commemorating military exploits.[57] On its walls hung the portraits of generals and statesmen who had taken part in the campaign, and Qianlong commissioned missionary artists to produce sixteen scenes depicting important battles and memorable events of the war.[58] After the emperor saw some engravings of original battle paintings done by the German painter Rugendas (1666–1742), he decided to have his sixteen paintings engraved in copper.[59] This art had been forgotten in China, and missionaries cooperated with French merchants in Canton to send the pictures to Paris for engraving, where the sinophile Bertin helped arrange for the work to be done by the best craftsmen of the day and for extra copies to be made for the French king and his senior ministers, despite a stipulation of exclusivity for the Chinese. After the engravings were returned to China, they were copied,

[56] Staunton, *Authentic Account*, 3: 11, 53, 143; see also *Daqing Gaozong Shilu, juan* 1493, 16b-18b, 60/12/25.

[57] Qianlong personally commemorated his armies' victories in a 1792 essay titled *Shi Quan Ji* [Record of Ten Complete (Victories)]. From this work, he also took as a literary name the appellation "Old Man of the Ten Complete [Victories]" (*Shi Quan Lao Ren*). Arthur W. Hummel, ed., *Eminent Chinese of the Ch'ing Period (1644–1912)*, 2 vols. (Washington, D.C., 1943), 369. See also Zhuang Jifa, *Qing Gaozong Shi Quan Wugong Yanjiu* [An Investigation into the Ten Great Victories of the Qianlong Emperor] (Taibei, 1982).

[58] The saga of the victory pictures has been exhaustively studied by Western scholars: see Paul Pelliot, "Les conquêtes de l'empereur de Chine," *T'oung Pao*, 20 (1921): 180–274; Michèle Pirazzoli t'Serstevens, *Gravures des conquêtes de l'empereur de Chine K'ien-long au musée Guimet* (Paris, 1969); and Hartmut Walravens, *China illustrata: Das europäische Chinaverständnis im Spiegel des 16. bis 18. Jahrhunderts* (Weinheim, 1987), 40–56. See also Hao Zhenhua, "Lang Shining Zhongguo Xiyu Zhantu de Shishi ji Yiyi" [The Historical Circumstances and Significance of Castiglione's War Paintings of the Qianlong Emperor's Campaign against the Zunghars in the Northwestern Border Region], *Sino-Western Cultural Relations Journal*, 13 (1991): 18–32.

[59] Qianlong expressed interest in European battle pictures more than once. In conversation with Father Benoist in 1773, he said, "There are a number of European prints that represent [military] victories won by your sovereigns. Who are they defeating, what enemies have they had to fight?" *LEC*, 13: 427.

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somewhat less elegantly, by Chinese craftsmen trained in the meantime in the technique of copper engraving by one of the Jesuits.[60] That this exchange played no part in subsequent European judgments of China can probably be explained by the likelihood that the details of this exchange were buried in Paris along with the *ancien régime* and that Frenchmen in China who had helped expedite it had little interest in recalling it as a counterweight to other views.

Political events in Europe were indeed the main reason for the shift in European attitudes toward China. In the earliest phases of Sino-Western interaction, Jesuit missionaries were virtually the sole interpreters of China to Europe. Because they generally wished to gain credit by conveying the civilized nature of the millions they expected to convert to Christianity, their accounts tended to be strongly approbatory. During the course of the eighteenth century, however, the steady decline of the once-powerful Society of Jesus, culminating in the order's formal abolition in 1773, radically undermined Jesuit credibility and prompted a new wariness among missionaries regarding what they chose to report. At the same time, the trend toward secularization in Europe was matched by the eclipse of the Jesuit monopoly on information. Various embassies, merchants, and travelers began to visit China and to broadcast opinions that were often quite unfavorable, in part because of China's continuing unwillingness to welcome foreigners except on its own terms.

But Chinese restrictions were not the only or even the primary reason that Western descriptions of China grew increasingly adverse. The far-reaching intellectual and political developments of the eighteenth century combined with advances in the quest for technological supremacy to exercise a pervasive influence on the European interpretation of other cultures. For example, China's great agrarian accomplishments that the French Physiocrats once vaunted seemed less admirable in a budding industrial age; the restrictive Canton system of trade went directly against the free world market advocated by Adam Smith in 1776, and the absence of political liberty or consensus in China did not sit well with the revolutionaries who dominated the latter part of the century.[61] Finally, the increasing predominance of negative Western attitudes toward other cultures at this time also partly reflected the disdain Westerners felt when they compared the relatively class-bound societies of traditional Asia and Africa with the dynamic social changes of their own postrevolutionary, industrializing societies.[62]

In the late eighteenth century, well before the Western incursion brought a new immediacy to the need for military reform, the Chinese were extremely interested

[60] Subsequent wars were commemorated in victory pictures drawn or engraved in China by Chinese artists and craftsmen. These included the Muslim uprising at Ush in 1765, the Second Jinchuan War of 1771–76, the Vietnam (Annam) campaign of 1788–89, the Taiwan rebellion of 1787–88, the 1792 war in Nepal, the campaigns to suppress the Miao minority in Yunnan and Hunan during the 1790s, the war against Jahangir in Xinjiang in the 1820s, and the Muslim wars of the late nineteenth century. For the identity of the Chinese artists, see Walravens, *China illustrata*, 42; Pirazzoli t'Serstevens, *Gravures des conquêtes de l'empereur de Chine*, 43. On the history of some of the indigenous victory pictures, see also Pelliot, "Les conquêtes," 243–44.

[61] See Jonathan D. Spence, "Western Perceptions of China from the Late Sixteenth Century to the Present," in Paul S. Ropp, ed., *Heritage of China: Contemporary Perspectives on Chinese Civilization* (Berkeley, Calif., 1990), 1–14.

[62] Adas, *Machines as the Measure of Men*, esp. 166–67, 178–79, 195–98. See also McNeill, *Pursuit of Power*, esp. 185–222.

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in technological advances and in what the West had to offer. The evidence was readily available to Europeans who chose to grasp it. Yet when in public the Chinese denied such an interest, primarily for reasons of domestic politics, Europeans, similarly influenced by developments at home, took that denial as evidence of an entire mental attitude: ingrained xenophobia and a concomitant resistance to progress. In the Age of Progress, such an attitude led automatically to the assumption that the Chinese were inferior beings.

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Review Article

Objectivity *and* Relativism Affirmed: Historical Knowledge and the Philosophy of Charles S. Peirce

JAMES HOOPES

> It appears that there are certain mummified pedants who have never waked to the truth that the act of knowing a real object alters it. They are curious specimens of humanity, and as I am one of them, it may be amusing to see how I think.
>
> Charles Sanders Peirce[1]

Joseph Brent, **Charles Sanders Peirce: A Life** (Bloomington: Indiana University Press, 1993), 388 pp., $35.00.

Charles Sanders Peirce, **Reasoning and the Logic of Things**, Kenneth Laine Ketner, ed., intro. by Ketner Putnam and Hilary Putnam (Cambridge, Mass.: Harvard University Press, 1992), 297 pp., $22.50.

INTELLECTUAL HISTORIANS have long lamented the absence of a useful biography of Charles Sanders Peirce (1839–1914). Taken together, Joseph Brent's well-written narrative of Peirce's tormented life and Kenneth Laine Ketner's edition of Peirce's 1898 lectures, *Reasoning and the Logic of Things*, which has some of the qualities of an intellectual autobiography, constitute an outline of that much-needed biography. These books will add to Peirce's increasing acclaim as the greatest American philosopher and, indeed, as Sir Karl Popper said, "one of the greatest philosophers of all time."[2]

Reasoning and the Logic of Things opens with a brief glimpse of the meditations on classical, medieval, and early modern philosophy that contributed substantially to Peirce's own achievements—an intriguing relationship between historical study and contemporaneous practice. There follow descriptions of his youthful passion for formal logic, of his pathbreaking conclusion that formal logic has nothing to

Many of the ideas in this essay and, in some instances, the words in which they are expressed are drawn from a longer yet-to-be published essay on historical methodology written jointly by my colleague Steven Collins and me. Professor Collins disagrees with me that he is entitled to claim joint authorship of this essay, but I must at least acknowledge my indebtedness here. I am also grateful to Thomas Bender and George Cotkin for several suggestions concerning this essay.

[1] "The Basis of Pragmaticism," in James Hoopes, ed., *Peirce on Signs: Writings on Semiotic by Charles Sanders Peirce* (Chapel Hill, N.C., 1991), 255.

[2] Karl R. Popper, *Objective Knowledge: An Evolutionary Approach* (Oxford, 1972), 212.

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do with any supposedly subjective feeling of reasonableness, and of his enormously important creation of symbolic logic or, as he called it, "the logic of relatives." Along the way, there is a brief account of the reasoning by which he concluded that "logic is itself a study of signs. Now a sign is a thing which represents a second thing to a third thing, the interpreting thought."[3] Peirce also explains how he generalized this triadic, semiotic conception of logic into his metaphysical categories of firstness, secondness, and thirdness. He pushes on to reflections on causation and force that may well remain the ablest refutation yet written of the philosophical skepticism and gargantuan scientism of the (then) impending twentieth century. His concluding lectures "Habit" and "The Logic of Continuity" make clear the relationship between his youthful work in metaphysics and the evolutionary, religious cosmology that occupied his later years.

Brent's *Life* of Peirce says little of significance about Peirce's thought and some things that are mistaken. Brent calls Peirce an "absolute idealist," when in fact Peirce opposed the whole doctrine of subjective ideas as it had been developed by Descartes and Locke, the tradition out of which absolute idealism grew. Brent writes that Peirce sought to move "beyond the sign, thoughtless, into the experience itself" when in fact Peirce believed representation was "experience itself."[4] Peirce's categories, which he believed to be his greatest achievement, are mentioned in a few places, but Brent offers no explanation of them or their importance. Peirce's numerous woes and shortcomings are recounted in detail but without the faintest suggestion of the fulfillment that he found in his philosophy, a fulfillment that can be felt in all he wrote. Brent's book is metaphysically dualistic, utterly un-Peirceian, old-fashioned intellectual history written out of a belief that life is basic to thought but not that there is life in thought.

Still, we must be grateful for this mine of information about the physical circumstances of Peirce's life. Brent gives us Peirce's intellectual running start as a child of Benjamin Peirce, professor of mathematics at Harvard and a leading algebraist of the nineteenth century. Had their proclivities been literary rather than mathematical and scientific, the Peirce family, with its brilliant but erratic father, four gifted sons, and one spinster daughter would probably be as intensely studied as the similarly constituted clan that produced William and Henry James, with whom the Peirce siblings were acquainted from childhood on. Benjamin, in addition to teaching at Harvard, was head of the U.S. Coast and Geodetic Survey, which provided Charles with employment as a working scientist for much of his life and made him one of the foremost geodesists of his time. Brent details Peirce's irresponsible dealings and consequent feuds with his employers: the Harvard Observatory, the Coast Survey, and Johns Hopkins, where for five years he held his only philosophic appointment. Here, too, are the disastrous marriages, the first of which, to an active feminist, ended in a divorce disturbing to middle-class sensibilities of the time. The second marriage, to a mysterious Frenchwoman who went by the name of Juliette Pourtalai and whose true name remains unknown, grew out of a love affair conducted with far less discretion than prevailing proprieties required. When the president of Johns Hopkins, Daniel Coit Gilman,

[3] *Reasoning and the Logic of Things*, 146.
[4] Brent, *Charles S. Peirce*, 206, 313.

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learned that the "marriage made no difference in the relations of the parties," Peirce's academic career was brought to an end.[5] Brent recounts charges of adultery and wife beating, along with indications that Peirce suffered from a facial neuralgia that eventually led to drug addiction. Combined with all these troubles, Peirce's delusions of powerful enemies, his grandiose dreams of a fortune around the next corner, and his insistence on a lavish lifestyle plunged him into the poverty and isolation of his later years.

In his later years, Peirce was dependent, sometimes reluctantly, on William James, who came to Peirce's aid by arranging monetary support, by crediting Peirce with the creation of the pragmatic philosophy that helped to make James famous, and by arranging lectures, including the 1898 series in Cambridge. (The depth to which Peirce's reputation had fallen by 1898 may be measured by the fact that James did not even attempt to have Peirce speak on the Harvard campus; President Charles William Eliot had denied earlier such requests on Peirce's behalf.) Peirce responded gratefully to James's aid and even adopted in old age the middle name "Santiago" (Saint James) as a tribute to his benefactor. Yet he also changed the name of his philosophy from pragmatism to "pragmaticism" to express his dismay with what James and other popularizers had done to his ideas. In preparation for the 1898 lectures, James had urged Peirce to abandon the mathematical elements of his philosophy in favor of "topics of a vitally important character."[6] In the first lecture, Peirce asserted that the idea of "*vital importance* seems to be a very low kind of importance" as compared to the mathematician's pursuit of the "real potential world."[7] And "A Neglected Argument for the Reality of God," Peirce's 1909 essay and almost his last philosophical breath, ended with an implied rebuke of James for "such confusions of thought as that of active willing (willing to control thought, to doubt, and to weigh reasons) with willing not to exert the will (willing to believe)."[8]

ONE OF THE POSSIBILITIES opened for intellectual historians by the new Peirce scholarship is a recognition that there were two pragmatisms, not one. To employ contemporary terminology anachronistically, there was the foundationalist pragmatism of Peirce on the one hand and, on the other, the anti-foundationalist pragmatism of James and Dewey. The latter, somewhat inaccurate, label refers to the efforts of a current leading philosopher, Richard Rorty, to claim the sanction of James and Dewey for his attempt to philosophize without foundations, without attention to the epistemological or ontological basis of knowledge and reality. Thus Rorty deplores the "tendency to overpraise Peirce," whose "contribution to

[5] The quotation is from Gilman's informant and Peirce's Hopkins colleague, the distinguished scientist Simon Newcomb. Interestingly, Newcomb was Arthur Conan Doyle's model for Professor Moriarty, the dastardly opponent of one of fiction's great semioticians (*Boston Globe*, January 5, 1993, 3). For Newcomb's real-life treachery, see Brent, *Charles S. Peirce*, 151–52.

[6] James's letter of December 22, 1897, quoted in Ketner and Putnam, "Introduction," *Reasoning and the Logic of Things*, 25.

[7] *Reasoning and the Logic of Things*, 121.

[8] *Peirce on Signs*, 278.

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pragmatism was merely to have given it a name, and to have stimulated James."[9] Yet, as Rorty's negative statement might lead one to suspect, for some time Peirce's star has been ascending faster and higher in some philosophical circles than James's or Dewey's.[10] In any case, Dewey and James did address, however unclearly, foundational issues, so it is not quite accurate to invoke them in the anti-foundationalist cause. In fact, James and Dewey, like Peirce, arrived at a metaphysical universe of relations. Dewey, however, never offered an account of these relations, failing especially to explain whether they were dyadic, triadic, logical, representational, or mechanical.[11] James concluded that the fundamental relations of the universe were dyadic, but, unable logically to reconcile dyadic relations with the metaphysical continuity in which he believed, he decided simply to "give up the logic" that made them seem contradictory.[12] A recent important study showed that James and Dewey's emphasis on relations was important to the social vision of twentieth-century American liberalism. But in view of Dewey's vagueness and James's illogicality on the question of relations, it is at least debatable that we ought to "build bridges back" to them.[13]

Although Peirce has played almost no role in twentieth-century political and social theory, he may well be the thinker toward whom political theorists ought to look back, for the well-known communitarian implications of his philosophy were based on his profound categorical analysis of relations. He early rebelled against the atomistic individualism that rested on Descartes' dualistic description of the universe as divided into the two substances of matter and mind. In Descartes' system, a human being was formed by the conjoining of the physical body and the individual soul or mind. Peirce rejected Descartes' two substances in favor of one, which was his metaphysical category of *firstness*. Peirce's one substance was capable of two different kinds of relations. A physical relation or *secondness* is the relation that occurs when one object is affected immediately by another, a second. A mental or spiritual relation—*thirdness*—occurs when one object is represented to another by a third, a sign. This notion of thought as a semiotic relation of the same elements involved in material events is profoundly social. It means that the "individual" human self is not an atomistic soul but is a semiotic (even "social") relation of the body's physical parts.

These semiotic relations, moreover, are scarcely limited to the self but may be extended between and among selves by language and other sign systems, which would make thinking a social as well as an individual process. Conversely, there is a sense in which society no less than its individual citizens may be considered a self. Group spirit, nationalism, and Christian fellowship "are no mere metaphors. None of us can fully realize what the minds of corporations are, any more than one of my brain cells can know what the whole brain is thinking. But the law of

[9] Richard Rorty, *Consequences of Pragmatism: Essays, 1972–1980* (Minneapolis, Minn., 1982), 160, 161.

[10] Eugene Freeman, ed., *The Relevance of Charles Peirce* (La Salle, Ill., 1983), *passim*.

[11] Raymond D. Boisvert, *Dewey's Metaphysics* (New York, 1988), 36–42.

[12] William James, *A Pluralistic Universe* (1912; Cambridge, Mass., 1977), 96.

[13] James T. Kloppenberg, *Uncertain Victory: Social Democracy and Progressivism in European and American Thought, 1870–1920* (New York, 1986), 11.

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mind clearly points to the existence of such personalities."[14] Yet because of the difference in the secondness out of which citizens and society are composed (human bodies in the one case plus instruments of communication in the other), there is less danger of losing sight of the difference between individuals and groups than in vague, organic notions of society. The implications of such views for democratic theory are potentially momentous and almost entirely unexplored.

It was in opening up such hitherto-unexplored paths of inquiry that metaphysics could be of use, according to Peirce. He admitted that it was extremely unlikely that metaphysical truth could ever be "brought to the test of experience" but held that metaphysics might fall in with science "as in inward harmony with it, obeying its logic, and serving its turn."[15] For historians, the test of his metaphysics must lie in whether it helps make history more comprehensible than either the Cartesian dualism he attacked or the romantic postmodernism that the pro-modern Peirce would have opposed and in whose cause his name is sometimes mistakenly invoked.[16]

THIS NEW UNDERSTANDING OF PEIRCE should therefore prove especially interesting to intellectual historians and readers of the methodological discussion that has been carried on in the "Forum" and elsewhere in the pages of the *AHR*. Most of the participants in the *AHR* debate, and, indeed, most intellectual historians today, remain trapped in vestiges of Cartesian dualism or its offshoot, metaphysical idealism. Postmodernists idealistically attempt to describe experience, context, and social structure in terms of thought, meaning, and language. Contextualists, on the other hand, posit a dualistic relation between thought and reality. Occasionally, someone asserts that thought is an aspect of reality, that meaning and language are aspects of experience and context. But the disputants offer no foundational examination of this proposition and, accordingly, slide back almost unwittingly into a dualism of mind and matter, meaning and experience. The "linguistic turn" has led to a dead end not because it has radicalized historical investigation but because it has reinforced the false dichotomy between words and objects, thought and reality, text and context that prevailed before the turn.

The 1987 essay by John Toews, "Intellectual History after the Linguistic Turn," contains the vestigial dualism past which six years of methodological discussion has not carried us. His recourse to traditional dualism in which mind represents matter and meaning represents experience is evident in his statement that intellectual historians have to sustain "the dialectical unity of and the difference between meaning and experience (as all historians must) in the wake of the linguistic turn."[17] His protest against separating signifier from signified was meant to maintain relations between the dualistic categories of mind and matter.

[14] Peirce, "Man's Glassy Essence," *Collected Papers of Charles Sanders Peirce*, Charles Hartshorne and Paul Weiss, eds. (Cambridge, Mass., 1931–58), 6: 271.

[15] Peirce, *Reasoning and the Logic of Things*, 109, 117.

[16] Jacques Derrida, *Of Grammatology* (Baltimore, Md., 1976), 49.

[17] John E. Toews, "Intellectual History after the Linguistic Turn: The Autonomy of Meaning and the Irreducibility of Experience," *AHR*, 92 (October 1987): 882.

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Thus it was dualism that led him to caution against "a semiological theory of meaning in its extreme form."[18]

David Harlan's subsequent *AHR* essay, which some read as an embodiment of the semiological extremism against which Toews cautioned, is actually no less traditionally dualistic than Toews's. Harlan attacked "radical contextualists" for their failure to acknowledge that there is no "structured system generating fixed meanings" but "only the incessant and unremitting play of signifiers—signifiers freed from the tyranny of the signified." Yet he claimed that texts *can* be interpreted according to "a particular historical tradition—not the tradition in which the text was written (we can never recover that) but the tradition of interpretation that has grown up around the text since it was written."[19] Harlan's confusion goes deeper than the obvious contradiction—noted by Joyce Appleby—of claiming that one tradition can be recovered but not another.[20] To claim that it is possible to recover the tradition in which a text has been interpreted but not the tradition in which it was written is to imply that, while we cannot know historical reality, we nevertheless have a wonderful ability to know what has been thought about that reality. To make the assertion that thought can be known but reality cannot is to lapse into the very Enlightenment epistemology that the poststructuralists claim to surmount.

The *AHR* discussion of historical knowledge has lacked philosophical substance partly because of the fashionable tendency to substitute for argument citation of the like-mindedness of the "community" to which one belongs. Harlan offered no reasons for his views but simply cited the names of Derrida and Foucault, as if their having stated their convictions was also a proof of them. David Hollinger, responding to Harlan, relied not on philosophical argument but on "examples of existing historical scholarship."[21] James Kloppenberg subsequently attempted to provide philosophical justification for reliance on shared practice by citing Dewey and James: "Whereas Peirce sought to find a logic of inquiry that would eventually but inevitably yield Truth, James and Dewey placed their confidence in free-floating communities of inquiry whose results could never achieve that status."[22] Lest anyone read this as a one-sided endorsement of relativism, Kloppenberg also endorsed objectivism, as is demonstrated by his reliance on words such as "disproved" and "verified," albeit "provisionally." Desiring to find an "alternative to the categories of objectivism and relativism," Kloppenberg actually offered up

[18] Toews, "Intellectual History after the Linguistic Turn," 882.

[19] David Harlan, "Intellectual History and the Return of Literature," *AHR*, 94 (June 1989): 594, 582, 588.

[20] Joyce Appleby, "One Good Turn Deserves Another: Moving beyond the Linguistic; A Response to David Harlan," *AHR*, 94 (December 1989): 1329.

[21] David A. Hollinger, "The Return of the Prodigal: The Persistence of Historical Knowing," *AHR*, 94 (June 1989): 621.

[22] James T. Kloppenberg, "Objectivity and Historicism: A Century of American Historical Writing," *AHR*, 94 (October 1989): 1026. The idea of a community of inquirers was Peirce's long before it was James's or Dewey's, although Peirce would have rejected any notion that such a community could be "free floating" if that phrase implies an antifoundational unconcern for ontological reality. According to Peirce, the fact that reality is constraining is what makes it possible to rely, not on any individual, but on an infinitely inquiring community, eventually to find the truth about reality. See Peirce, "Consequences of Four Incapacities," *Writings of Charles S. Peirce: A Chronological Edition*, Christian J. W. Kloesel, *et al.*, eds. (Bloomington, 1982–), vol. 2, 239; and Peirce, "How to Make Our Ideas Clear," *Writings of Charles S. Peirce*, 3: 273–74.

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the same compromise between them that, as he himself pointed out, many historians have relied on for a century. Kloppenberg's instincts soundly led him to believe that he could have his cake and eat it, too, that historical knowledge can be both objective and relative. But had he attempted a philosophical argument rather than merely citing the views of James and Dewey, he might have found that Peirce offered the philosophically stronger, more useful variant of pragmatism.

There is not space here to pursue the history of pragmatism in detail, but there is a fundamental error in supposing that either James's or Dewey's was "a radical theory of knowledge that broke down inherited dualisms and replaced them with a conception of immediate experience as the intersection of subject and object."[23] James and Dewey did believe in "immediate experience," but as stated earlier this was far from a thoroughgoing attack on dualism. And their notion of "immediate experience" was not radical but grew out of the tradition of British empiricism in which the mind's knowledge of its ideas was considered immediate.[24] The epistemological radical of American pragmatism was not James or Dewey but Peirce, with his view that the mind's knowledge of both external reality and its own ideas is mediated through signs.

Peirce held it possible to accurately know the objective world because the very thoughts or signs by which we represent the world share a monistic or substantial identity with it. Like the world, thought must have firstness and secondness before it can have thirdness. Objects exist apart from thought, but it is a very different existence than they have in thought. It is scarcely radical to assert that the qualities of things in themselves cannot be known, but the way Peirce reasoned to this conclusion was radical. It is not, as Descartes, Locke, or Kant would have said, that thought cannot know objects in themselves. Neither is it, as Bishop Berkeley and other idealists would have said, that objects do not exist in the absence of thought. Rather, said Peirce, objects exist in the absence of thought, but in its absence they have no meanings or qualities to know. Although thought does not create objects, it gives objects the meaning and qualities that we rightly know them to have: "Modern nominalists are mostly superficial men, who do not know, as the more thorough Roscellinus and Occam did, that a reality which has no representation is one which has no relation and no quality."[25] Objective knowledge is relative because objective reality comes into being in its fullest in relation to thought.

The idea that knowledge is relative does not mean that it is subjective. There have been wild misreadings of Peirce's famous essay of 1878, "How to Make Our Ideas Clear," in which he defined truth and reality: "The opinion which is fated to be ultimately agreed to by all who investigate, is what we mean by the truth, and the object represented in this opinion is the real." Thomas Haskell glossed this as an "elegant epistemological rationale" for the "institutionalized authority" of experts: "Sound opinion becomes that opinion which wins the broadest and deepest support in the existing community of inquiry; there is, according to

[23] Kloppenberg, "Objectivity and Historicism," 1017.

[24] "For let any *Idea* be such as it will, it can be no other but such as the Mind perceives it to be"; John Locke, *An Essay concerning Human Understanding* [1689], Peter H. Nidditch, ed. (Oxford, 1975), 364. See also my book, *Consciousness in New England: From Puritanism and Ideas to Psychoanalysis and Semiotic* (Baltimore, Md., 1989), 40–41.

[25] *Writings of Charles S. Peirce*, 2: 239–40.

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Peirce, no higher test of reality."[26] This is the exact opposite of Peirce's view, in which opinion is tested by reality, not vice versa. In "The Fixation of Belief," an essay in the same series that included "How to Make Our Ideas Clear," Peirce dismissed subjectivism or "tenacity" because it did not allow for the fact that opinion is tested by reality, by "external permanency—by something upon which our thinking has no effect" but "which affects, or might affect, every man."[27] Reality is relative to thought in the sense that its qualities and meanings do not exist in the absence of thought. But although thought gives reality to qualities and meanings, it cannot arbitrarily change them. The sky's quality of blueness does not exist apart from our thought's interpretation of it as such, but we cannot arbitrarily make a blue sky brown.

PEIRCE'S PHILOSOPHY was not only the more logical and foundational of the two pragmatisms, it was also more useful to historians. For Peirce, history is meaningful not because of what human beings arbitrarily make of it but because meaning is literally in history. Historical events have a logic because thirdness or thought is an objective metaphysical category: "there is in the being of things something which corresponds to the process of reasoning, . . . the world *lives*, and *moves*, and HAS ITS BEING, in [a] logic of events."[28]

The notion of thirdness or thought as a basic category of the universe seems unscientific to the superficial tough-mindedness of the twentieth century, but Peirce, who had more experience as a working scientist than any other major philosopher, believed that the success of modern science could be accounted for in no other way. During another Cambridge lecture series in 1903, he cited gravity as an example of the reality of thirdness.[29] James attended Peirce's lecture but was evidently unpersuaded by it. In 1909, he explained his preference for dyadic relations by reflecting that no mediating thirdness need be supposed between his manuscript and the desk on which it lay because "the 'on' fails to appear to our senses" in the way that the desk and manuscript do.[30] But without the reality of some unsensed mediating thirdness such as gravity, authors seem astonishingly fortunate that in every known instance so far, manuscripts related dyadically to their desks have remained there rather than some of them drifting away to the moon or to Mars.

In the title of Peirce's 1898 lectures, he meant for the phrase *Logic of Things* to be taken literally. Logic is not a unique property of human beings but is found in

[26] Thomas L. Haskell, *The Emergence of Professional Social Science: The American Social Science Association and the Nineteenth-Century Crisis of Authority* (Urbana, Ill., 1977), 239. Haskell cautions against interpreting Peirce's view as a "cynical subjectivism" (nn. 237–38), even though such, in fact, is the import if not the intent of his interpretation. Haskell's influential book has thus wrongly helped make Peirce a buttress of the subjectivism and anti-objectivist relativism now rampant in the humanities and social sciences. Haskell has dealt more effectively with the question of subjectivism in a subsequent essay, although he finds his inspiration in Thomas Nagel rather than Peirce. See Haskell, "Rhetoric vs. Practice in Peter Novick's *That Noble Dream*," *History and Theory*, no. 2 (1990).

[27] *Writings of Charles S. Peirce*, 3: 253.

[28] *Reasoning and the Logic of Things*, 161.

[29] *Peirce on Signs*, 242–44.

[30] James, *Pluralistic Universe*, 41.

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the universe at large, which is why Peirce believed that the best training in reasoning comes from study of the external world rather than study of our own thoughts. Because meaning is in events, the historian should not attempt to impose meaning on history but only to discover it there, which is why the first paragraph of *Reasoning and the Logic of Things* is a critique of "that historical method which leads to the absolute and confident denial of all the positive testimony that is extant, the moment that testimony deviates from the preconceived ideas of the historian."[31] In view of the interpretive license at work in much of the history now written, Peirce's advice is even more useful now than in 1898: the "hypothesis . . . which we of these times ought to try is . . . that the logic of the universe is one to which our own aspires rather than attains."[32]

Peirce provides a philosophical basis for intellectual history by showing that there is a way to understand the historical importance of human thought other than by relating it to action. Thought is not something that must be somehow related to action. Thinking *is* a bodily action, a semiotic or triadic relation in the body or brain. Descartes puzzled over how the unextended substance of mind could influence the extended substance of matter and finally settled on a point of contact between mind and body in the pituitary gland. Peirce needed to make no such extraordinary claim, for he held that thought has the same substantial or "material" elements as a physical event. For Peirce, therefore, it is no conundrum that an insult may provoke a fist fight or propaganda a war. Peirce's semiotic corrects the assumption of some historians that thinking is not as "real" and potentially influential as other, seemingly more tangible elements in human life and society.

Neither did Peirce engage in the postmodernists' folly of retreating "from the signified . . . into the signifier" and ignoring the signified, the world of ontology, the world of bodily objects.[33] Because semiosis or thirdness requires firstness and secondness (that is, requires a body and its physical processes), thinking is a bodily action just as real as eating a meal and potentially just as historical as fighting a war. Similarly, Peirce's semiotic corrects the view of some, including many postmodernists, that thinking is an utterly free process, unfettered by the relations in the natural world that constrain other kinds of human activity. Peirce's semiotic therefore allows for realistic recognition that human life and society are to a significant degree a matter not only of freedom but also of constraint, a matter of people being shoved this way or that by bullets and ballots, a surplus or shortage of land, the rise and fall of technologies and industries.

Yet Peirce's semiotic does allow for some human freedom or, rather, a role for thought. Because thought is in signs requiring subsequent interpretation in order to achieve meaning, a human being may exercise some but scarcely complete control over his or her thoughts. There is a sense, then, in which Peirce's semiotic as well as that of the postmodernists locates the creation of meaning outside of

[31] *Reasoning and the Logic of Things*, 106. For a similar contemporary view, see Umberto Eco, "Overinterpreting Texts," in Eco, *et al.*, *Interpretation and Overinterpretation*, Stefan Collini, ed. (Cambridge, 1992), 45–66.

[32] *Reasoning and the Logic of Things*, 258.

[33] See Anthony Giddens's lucid critique of postmodernism in *Central Problems in Social Theory: Action, Structure and Contradiction in Social Analysis* (Berkeley, Calif., 1979), 37–38.

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human intention.[34] But it is precisely here that Peirce's analysis of the signifying process as containing three *related* elements—object, sign, and interpretant (the thought that interprets the sign as a representation of the object)—helps us to make sense of the fact that there is some, though of course far from perfect, correlation between what people say they plan to do and what they then do. Postmodernists cannot offer us such comprehension because of their dyadic, ahistorical signifier/signified disjunction. In Peirce's analysis, the meaning of the sign is determined partly by its real relation to the object and is thus partly unintended by the human agent. But the meaning of the sign is also determined partly by its relation to the interpretant, by what the human being makes of the sign in a subsequent thought. Thus, one meaning of Peirce's semiology for historical practice is that neither present nor past people's thoughts can be dismissed as irrelevant to the rest of their behavior. Peirce's metaphysics makes it possible to understand how there could be such a thing as the human purposefulness that historians, whether influenced by Cartesian dualism or by postmodernism, find difficult to acknowledge as a historical force.[35]

Peirce's semiotic has a still larger advantage for historians in that it makes it possible to conceive of the study of the past as possible and intellectually valid. The signifier/signified disjunction of the postmodernists cuts off the possibility of historical knowledge by suggesting that past people have no connection with the documents they have left us. The "author's presence" is, in Harlan's words, "always deferred, always elsewhere, always already absent."[36] But in Peirce's semiotic, the author's self is neither entirely present nor entirely absent from the historical document. The document or text has a relation to its creator that resembles, in one respect, its relation to the historian. The document's creator had no more unmediated, unrepresented access to his or her thoughts than does the historian. For both the document's creator and the historian, the text's meaning was and is signified by a real relation with not only its object but also its interpretant. Intellectual historians are, therefore, in one respect, on exactly the same level as people of the past; we are interpreters of their thoughts. Because past people no more than present people had immediate or unrepresented knowledge of their thoughts, the text or document was for them and is for the

[34] This was one of the reasons for Toews's original caution against semiotic extremism; "Intellectual History after the Linguistic Turn," 882.

[35] This practical example of the usefulness of metaphysics gains weight by comparing it with Joyce Appleby's reliance on mere common sense to answer the same question of the relationship between language and human intentions. As writers themselves, historians "know," says Appleby, that they are "animated by intentions"; "One Good Turn Deserves Another," 1330. But in the absence of any explanation of how such self-knowledge is possible, Appleby's assertion begs the question; she relies on the very notion that the postmodernists (and Peirce) question: that the meaning of human experience is self-evident and transparently immediate. Such argumentation is a good example of what is most likely to happen when one "free-floating community" (in this case, contextualists) encounters another (postmodernists). Rorty suggests that we do not need foundational discussion "to keep our culture moving," and that is probably true. Richard Rorty, "Comments on Sleeper and Edel," *Transactions of the Charles S. Peirce Society* (Winter 1985): 40. But if we wish for our culture not merely to move but to make it possible for us to communicate with each other, foundational discussion is unavoidable. In the absence of discussion of the foundational questions that separate them, contextualists and postmodernists cannot engage each other but merely hurl assertions and counter-assertions, a linguistic turn indeed.

[36] Harlan, "Intellectual History and the Return of Literature," 592.

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historian a sign that may be correctly or incorrectly interpreted. But the document or sign of the past that the historian interprets is the same that was interpreted by its creator, which opens up the possibility of a relationship that postmodernists deny and that J. G. A. Pocock has affirmed, the possibility of "a communication with the author's Self."[37] There is no necessary contradiction between the view that we live in a universe of signs and the view that the search for historical knowledge is a risky but possible search for objective truth.

[37] J. G. A. Pocock, *Virtue, Commerce, and History: Essays on Political Thought and History, Chiefly in the Eighteenth Century* (New York, 1985), 25. Compare Harlan, "Intellectual History and the Return of Literature," 592; and Hollinger, "Return of the Prodigal," 621.

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Featured Reviews

PHILIPPE CARRARD. *Poetics of the New History: French Historical Discourse from Braudel to Chartier.* (Parallax Re-visions of Culture and Society.) Baltimore: Johns Hopkins University Press. 1992. Pp. xix, 255. $32.95.

In this work, Philippe Carrard, a professor of French, looks at the *Annales* tradition of historiography in the light of literary theory—specifically, theory of narration, theory of enunciation, rhetoric, and stylistics. Sometimes he reveals, but with greater clarity, things that informed historians will have already seen vaguely. Sometimes what he says will surprise all but cognoscenti, as when he discusses the New Historians' use of quotation marks and the presence of different levels of language in their works, or when he attends to the relations between gender and historical research and writing.

In his first chapter Carrard offers an account of the tradition against which the *Annales* historians defined their work, the "positivist" historiography for which Charles-Victor Langlois and Charles Seignobos's *Introduction aux études historiques* (1898) served as a manual. Positivist historiography focused on events, whereas the *Annales* historians turned to "existents" (geographic regimes, economic systems, persistent practices in general), asking not merely "What happened?" but the broader question, "What was the case?" Positivist historiography also had a prescribed mode of writing: historiography ought to be chronological in organization, objective in enunciation (with the historian not intruding into the text), and literal in style. Carrard shows, by examining several works in the "positivist paradigm," that its practitioners did not uniformly follow the prescription.

But the main contribution of the book lies in Carrard's discussion of the theory and practice of the *Annales* historians themselves. His second chapter focuses on narrative, defined in the conventional way, as an account organized according to temporal sequence. Because of a widely noticed article by Lawrence Stone, "The Revival of Narrative" (*Past and Present* 85 [1979], 3–24), many historians will have at least a vague familiarity with the subject. Carrard shows that the notion that the New Historians were engaged in a "revival" of narrative in the 1970s is almost entirely mistaken. On the one hand, despite their programmatic opposition to *histoire-récit*, the *Annalistes* remained remarkably attached to narrative in practice. On the other hand, some of the works touted as instances of narrative's revival—most notably, Emmanuel Le Roy Ladurie's *Montaillou, village occitan de 1294 à 1325* (1975)—were not narrative. By defining narrative clearly and then looking at specific works with this definition in mind, Carrard brings welcome clarity to a matter that discussions among historians have almost always tended to obscure.

In his third chapter Carrard focuses on the New Historians' modes of enunciation and on their implied audiences. Like their predecessors, the New Historians aimed, in theory, at objectivity in enunciation. But their practice, Carrard shows, is more complicated. In some cases they are almost flamboyantly present in their works. Carrard finds some correlation between the degree of institutional standing that a historian enjoys and divergence from objective enunciation. He also finds that women historians seem more inclined to follow objectivist rules, perhaps because they fear being judged in the light of stereotypes about female subjectivity. In general, the implied audience of works in the New History is relentlessly academic. Although New Historians do attempt popularizations, the claim sometimes made that the New History has "best seller" status in France is generally not confirmed by the sales figures that Carrard has obtained (pp. 136–37). Although *Montaillou* sold 188,540 copies through January 1989, its sales were vastly atypical. Closer to the norm is the classic text of the *Annales* school, Fernand Braudel's *La Méditerranée et le monde méditerranéen à l'époque de Philippe II*, which in its two French editions sold only 8,500 copies between 1949 and 1985. The public impact of the *Annalistes* in France was a function much more of their institutional and media connections than of their readership.

Finally, in chapter 4, Carrard turns to the strategies

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and devices that New Historians use to persuade readers that their accounts are true (rhetoric) and to give their texts some measure of readability (stylistics). He analyzes New Historians' use of evidence, their use of notes, the role of quantification in their works, and several other topics. He shows how, in the situation of uncertainty that is theirs (and ours), "eminent scholars seek to reconcile the diverging demands of their field" not by tight adherence to "logic" but by a loose strategy of "coping." "Coping," he suggests, is "the only available option when codes are no longer perceived as authoritative" (pp. 181, 181–98).

Carrard has written a book that historians can read with profit. So can literary theorists. Because of influential essays by Émile Benveniste, Roland Barthes, and Julia Kristeva, it is widely assumed in literary theory that "historical discourse" is the acme of plain, impersonal, neutral, objective discourse. Carrard shows that this view is based on a hasty reading of "positivist" historiography, which is then taken as definitive of "historical discourse" generally. Further, he makes the point that there is no such thing as "*the* discourse of history." His observation accords with a fairly general perception now that the discipline is marked by fragmentation and that *histoire totale* (in the strong sense of that project) is dead. Carrard's book is interesting in part because it is a model of the sort of commentary that, in the face of multiplicity on the levels of both content and method, offers one of the few ways of seeing the historical endeavor whole. Viewed more narrowly, Carrard's book is an excellent introduction to the *Annales* tradition in historiography, an indispensable complement to studies by such historians as Traian Stoianovich and Peter Burke.

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Lewis Perry. *Boats against the Current: American Culture between Revolution and Modernity, 1820–1860*. New York: Oxford University Press. 1993. Pp. xii, 332. $35.00.

"There are no second acts in American lives," F. Scott Fitzgerald wrote in his last, unhappy year. The same might be said of many once brightly spotlighted American historiographic traditions. Of these none has been more comparatively darkened in recent years than the post–World War II American Studies works that dramatized antebellum culture as the seedbed of American identity. Lewis Perry's intriguing book reopens the curtain with a flourish. Yet Perry's study differs in important respects from these previous works. Certainly he is more cautious. He avoids speaking totalistically about symbols for an age while pointedly including voices, particularly those of women, largely absent in classic American Studies writings. Another, closely related difference is Perry's sociological emphasis. The grand intellectual and literary configurations of traditional American Studies practitioners hover noiselessly around the wings while social experiences and professional concerns occupy the center stage. Finally, Perry's approach to the relation between highbrow and popular culture differs from that of most of his precursors. Henry Nash Smith, John William Ward, R. W. B. Lewis, and Leo Marx all sought to reveal certain partly ambivalent but still basically absolutist stances of their popular cultural actors—whether toward the West, democracy, or America's Adamic vision—to which they counterposed, either explicitly or implicitly, a confident, ironically modernist critique often worked out through the texts of major American Renaissance authors. Perry, by contrast, sees all his figures, high and low, in varying degrees drifting or earnestly rowing in the shifting stream of his subject.

That subject is "modernity," by which he means "both progressive improvement and uncertainty of conviction" (p. 215). Furthermore, Perry tells us, modernity in American culture was originally born not in twentieth-century modernist cultural debates but in "the fluidity of society in Jackson's epoch" (p. 7). To introduce this "familiar" era Perry begins, appropriately enough, in a section entitled "Nashville," with the tragico-comical opening scene from Fitzgerald's Great Depression era–set novel, *The Last Tycoon*, in which a trio of rather desperate Hollywood movie people find their California-bound airplane temporarily grounded in the Tennessee city. Meanwhile they incongruously search for some sort of connection with a primeval but now absurdly distant American past through a visit to Andrew Jackson's Hermitage, while trying desperately to figure their next move in their unreal present and uncertain future. From there we move backward one century to find various travelers to Nashville cataloguing just the opposite movement—from, as they see it, barbarism to civilization—and, in the midst of a like current, the Old Hero himself. Perry's portrait reverses slightly the conventional Tocquevillian view of John William Ward and Marvin Meyers: instead of Jackson the adventurous conservative, we see Jackson the conservative adventurer fashioning for himself modern, democratic roles while gesturing toward traditional civility, integrity, and sentiment. But the primary purpose of Perry's Jackson is rather like that of Fitzgerald's grounded airplane—to introduce, in summary-narrative fashion, the moving cultural channels through which, Perry proceeds to show in

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the rest of the book, his fluid era flowed: history, travel, and theatricality.

Perry's discussion of history is the most clear-cut. The section is entitled "The Dead Past," and the title is apt. Dead past did not mean, Perry points out, no past. On the contrary, he chronicles the era's multiplying historical societies, nostalgic narratives of preindustrial communities, southern cults of medievalism, and alternately anxious and righteous invocations of the heroically virtuous republican ideals of the revolutionary "fathers." But he also finds these gestures variously freeze-dried, sentimental, bogus, or abstract. Meanwhile writers such as Ralph Waldo Emerson and Henry David Thoreau, despite the latter's fascination with Indians and mythologies, were out-and-out antihistorical, asserting the ever presence of all ages in the "now" and complaining that even their generation's weak veneration of the past was vitiating of creative life. Perhaps Perry goes too far. He mentions Francis Parkman and Washington Irving as two writers who protested in favor of Romantic conceptions of a living past, but he might have listed other Romantics who also did, from Transcendentalists such as Margaret Fuller and Orestes Brownson to conservatives such as Horace Bushnell and Philip Schaff. Furthermore, protests (which is how Perry flags Parkman and Irving), if carried over into actual works, create countercultures. (In this light it is significant that Perry cites Nathaniel Hawthorne's ambivalent comments on America's pastlessness but ignores the great fictional texts through which he expressed his deeply historicist consciousness.) Still, Perry's primary point is sound: for most American writers and pundits, history was either too far away or too near to constitute a living, organically influential past.

If the past was largely inaccessible to most American commentators, the changing present was palpably graspable, especially, Perry suggests, through the experience of travel, his second conduit of modernity. Although Perry's section on travelers is the social heart of his book, it is also methodologically the most problematical. Mainly this is because of the documentary approach that he sometimes uses: opinions are chonicled, compared to historical facts, and then dismissed as being ideologically biased. Although these are useful correctives that should satisfy hardheaded social historians, the result is a certain digressive movement that buries the intellectual thread of his argument. Also along the way intellectual bodies get distorted to fit ideological beds. It is simply not true, for example, that Alexis de Tocqueville's speculations about American philosophy and poetry are entirely negative and "notoriously wrong" (p. 95) when measured against the later examples of Emerson, Whitman, and Emily Dickinson. On the contrary, his remarks about democracies encouraging poets to find in the human heart and inner soul, rather than external senses, the sources of vast beauty, much as Europe's great modern Romantic writers had been doing, show both surprising sympathy and extraordinary prescience.

For the most part, however, Perry pursues with great ingenuity his objective of treating his travelers as constituting a culture by striving through their travels to attain knowledge, professional success, and social influence while participating in an international conversation "about the development of civilization" (p. 8). Perry is particularly good at showing the creative tension between his figures' exiled status as travelers and, apart from the partial exception of the bitingly pastoral Thoreau, their discovery of American progress. Perry's social perspective also allows him to make new discoveries of his own. His counterpoint between the two Europeans, the culturally wary yet energy-conscious Tocqueville and the domestic-minded yet communitarian Fredrika Bremer, for example, brings out a freshly gendered dimension to a familiar ideological topic.

But it is with his third theme, on social theatricality, that Perry fully demonstrates his power to turn the familiar into the uncannily new. Antebellum Americans' preoccupation with sincerity and deception has recently been much discussed by cultural historians. What Perry adds are two vital dimensions: that despite their obsession with internalizing fundamental truths, many middle-class Americans displayed an extraordinarily protean capacity for changing roles, donning masks, and speaking in multiple voices, and further, that this role-playing and dramatic "equivoke" was directly related to their experiences peddling goods and reforms, finding markets and audiences, and judging the sincerity or insincerity of other like-minded unknown and ultimately unknowable travelers on the road. Nor were these only the gullible or the deceiving. In a brilliant chapter entitled "Emerson on the World of Shows," Perry provides perhaps the best show of all: a picture of the American nineteenth-century's greatest poet-critic appealing to both self-trust and the multiplicity of selves to be trusted while using each to self-consciously critique and dramatize the other. Yet even here we are left with an ironic picture of Emerson the radical-conservative, traveling from pulpit to lecture hall, seeking to generate a national drama out of self-drama, but doing so to a venerating but only half-hearing audience anxious for material success and cultural respectability.

Perry concludes his book with a dual summation of his themes of modernity. In a cogent theoretical overview he takes particular pains to distinguish modernity's loose and ambivalent nineteenth-century concepts of change and uncertainty from the early twentieth-century's variously pragmatic-instrumental or avant-garde–subjectivist stances. The last two chapters, on the Garrisonian abolitionist Henry C. Wright and the Transcendentalist Theodore Parker, by contrast, are case histories: with each he cuts into a life in order to show a struggle for progressive change along with a contrary struggle to hold onto, whether

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(as with Wright) naively or (as with Parker) more self-consciously, the permanence of "truth."

Despite the ingenuity and subtlety of Perry's book, it will no doubt raise in the minds of many readers several problems. One is the question of causation. From time to time Perry indicates that modernity consisted during the antebellum years of a special erosion of "traditional" values. But exactly what these were, when they were, or what was the engine of transformation—whether democratization, capitalism, or some combination—is left unclear. (He mentions at one point "eras of hierarchical order," but leaves the American Revolution unaddressed, despite the book's subtitle [see pp. 8–9, 170–71]). Social historians might also want to know how much southern slaves, settled small farmers, Jacksonian artisans, and other groups participated in or were affected by Perry's culture of fluidity; when occasionally mentioned, they usually appear as distant objects of Perry's European or mostly Whiggish, northeastern adventurous travelers' observations.

Finally, one must question Perry's decision to relegate intellectual currents and literary forms to a minimal role in his narrative. Surely for many educated, middle-class people, Romanticism as much as the road shaped their cognitive struggles with change and uncertainty. Moreover, in shunting aside questions of formal art, as Perry does, one eliminates the area where antebellum culture moved closest to "modernism" proper. Equally important, one overlooks a critical set of self-referential, symbolical texts that, as the practitioners of the classic American Studies showed, can be used, not just as sources to mine for illustrative or narrative purposes (which is how Perry usually uses his literary texts), but also as powerful tools for exposing the subconscious feelings of an age. "It is hard to frame a story around people who felt the significance of their actions lay outside their own lives" (p. 269), Perry says at the end of his book. This is precisely an argument for looking hard at Romantic figures who in their texts looked out at least partly from within.

These difficulties notwithstanding, it should be clear that Perry has written an erudite, suggestive, and powerful book in the grand Tocquevillian tradition of American Studies. He has done for the road what Leo Marx did for the machine: taken a social fact and transformed it into a profound historical metaphor. Indeed, in one sense he has gone beyond American Studies historians in integrating his metaphor into the narrative strategies of his book. Just like his stream—his fluid version of the road metaphor—his narrative seemingly meanders at the beginning of each section, telling a story, suggesting a theme, picking up momentum, and rushing toward a final culmination, only to be undercut again with another story. Meanwhile we alternately drift or paddle hard through the thickets of his side stories, uncertain sometimes exactly where we are going. This method reaches an apotheosis at the end, in a charming epilogue, in which we are treated to a retelling of the story in a way that jumbles many of his characters and anecdotes, imagines or wishes for others that never appeared, and then stops. Through it all Perry keeps up the alternately earnest and playful tone of a narrator who wishes to hold on to moral and narrative commitments even while he knows much of what he sees are fragments. He exemplifies the very process he chronicles and, in so doing, has written an authentic Fitzgeraldian second act. How many historians in these drifting "postmodern" times dare as much?

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SHELLEY FISHER FISHKIN. *Was Huck Black? Mark Twain and African American Voices.* New York: Oxford University Press. 1993. Pp. xiv, 270. $25.00.

You don't know about me, without you have read a book by the name of "Was Huck Black?" but that ain't no matter. That book was made by Mrs. Shelley Fisher Fishkin, and she told the truth, mainly. There warn't no frivolishness about Mrs. Fishkin, not a bit. There was things she stretched, though that is nothing. I never seen anybody but told some stretchers, one time or another.

Now the way her book winds up is this: Long time past, Mr. Mark Twain took down some of my adventures and Mrs. Fishkin reckons in the reporting Mr. Twain told some big stretchers, nearly whoppers. He made my skin white but borrowed my talk from black folks without telling no one. Mrs. Fishkin told me that nothing couldn't be better and that cheered me up some. Except when I heard about Jim.

You remember Jim? He was Miss Watson's slave. Well naturly Mr. Twain made Jim's skin black but Mrs. Fishkin and a heap of other folks believe that Jim, he sometimes talked and acted white. Or most like white folks wished he would. Those with learning calls this trick minstrelsy. Not much good in this trick, I discovered, because there warn't no sassing nor cussing nor orneriness to it nor not enough grit.

Mrs. Fishkin says Mr. Twain never meant no harm by it. Just when those in school meet Jim today they feel all cramped up like the Widow Douglas made me feel in new clothes and I couldn't do nothing but

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sweat and sweat. I hope she don't take no offense but Mrs. Fishkin kind of reminds me some ways of the widow, she being so regular and decent in all her ways and trying to sivilize me and Jim. I judged her plan was to keep some of them professors and students from finding fault with us.

Mrs. Fishkin got out a heap of books and learned me about Jimmy, a black child who nearly wore Mr. Twain's ears off in some fancy hotel one time. Had a powerful influence. I was in a sweat to find out all about him. She says we talk alike. Repeating words, and tangling them all up, and unfixing their meanings, and using something she named a parcipial construction and ten shifters and castrated conjunctions and non-redundant tense markings and cereal verbs and double on the negatives and just sort of piling it on to beat the band.

Mrs. Fishkin worked me middling hard bout my speech and she kept pecking at me till I was most convinced I was Jimmy. Then I set down in a chair by the window and tried to think, but it warn't no use. It's like listening to an owl, away off, who-whooing about somebody that was dead. Mrs. Fiskin got ghosts on her mind, not the usual white kind, mind you, but black ghosts that can't rest easy in their graves and has to go about that book of hers grieving and a-ha'nting white folks.

Jim knows about ghosts, being bewitched and all and riding down to New Orleans and further sometimes so I asks him if I could be black, being as Pap was, best I remember, white to make a body's flesh crawl—a tree-toad, fish-belly white. And it got me feeling ruther blue, wondering about my Mam too, she dying and me so young.

But Jim, he jest laughs kind of soft and calls me honey and inquires if I been bewitched myself. I reckon learning is a kind of enchantment and Mrs. Fishkin and such like her—can call up the thunder and lightning with them genies in old books too. And those genies come tearing in and do everything they're told. People pay attention when them genies in books build a palace forty miles long and fill it full of chewing gum, or whatever you want, and fetch an emperor's daughter from China to marry. But I rubbed those books till I nearly sweated away and no genies came for me.

Mrs. Fishkin didn't ask them to do much for her, by my reckoning. She should have asked them to make Mr. Twain black, now that would have been something worth proclaiming. Cause then, as I take it, nobody would be blaming him for my ignorance. Now it was always hard to learn me anything though when I went to school it got easier. I felt kind of bad that I been using such a mean low-down word for old Jim but it warn't no fault of Mr. Twain. Some words ain't raspy on me like they is to folks today. It pulled on me pretty tight that I made it so warm for him.

Now Mrs. Fishkin pokes down further—down deeper—than even that King Solomon. She says that one book can't carry everything on its back. And that Jim warn't the only black voice that should be educating the likes of me. I reckon that the people that read her book knows what's the correct thing to do. I can't learn 'em anything. But I felt kind of lowdown and ornery and to blame somehow. But that's always the way. It don't make no difference whether you do right or wrong; a person's conscience ain't got no sense, and just goes for him anyway.

They say Mr. Twain don't hardly respect Jim for his being so gullible and scared and superstitious and all around foolish. I set down, one time, back in the woods, and had a long think about it. I asks myself, who warn't gullible and superstitious and foolish along the way? Pap, who was handy with his hick'ry and his cussing and them delirium tremens that come out of his jug. That judge who tried to reform the old man and him drunk as a fiddler in the judge's spare room. The King, a-prancing naked on all fours to a house jam full of men and the Duke, a-bragging about his great tragic performance, two low-down humbugs and the beatenest lot of frauds I ever struck, shellacking the greenhorns in all them Mississippi river towns, most hundreds of folks, until the tar and feathers hit 'em? Colonel Sherburn, him that shot the unarmed drunk for honor's sake, and the mob that went for him and then slunk away looking tolerable cheap? Tom Sawyer, whose plans were always worth fifteen of mine for sheer flapdoodle and gold-leaf distinctions with the added possibility of getting us all killed besides? The feuding Grangerfords and Sheperdsons who were pretty nigh killing the pack off on either side? No, says I to myself, there warn't no white person who warn't foolish except Emmeline Grangerford. Her brother Buck told me she could rattle off poetry like nothing. Emmeline Grangerford could match some of them professors in Mrs. Fishkin's book for style. Emmeline could write about anything you choose to give her to write about, just so it was sadful. She was easier to understand than some of them professors, but they warn't any deeper than her, no sir.

I most forgot the widow and Miss Watson. The widow she was awful good and not gullible. That's easy to show cause she took an interest in rapscallions and dead beats, always the way with good people. And Miss Watson was always trying to educate me. She said I was a fool. I expect she was right though she never told me why and I couldn't make it out no way except that she must have found out that I was going to hell on my own steam and didn't take no notion of playing with harps all day and singing forever. So there you have it. Only three in the whole story weren't blamed fools, and they was all women. I warn't feeling just right, before that, but it was all comfortable now. I was glad the way it come out, too. Mr. Twain, he was prejudiced alright, against the male race. And I could stand that cause it included me.

That Mr. Twain, he was always telling a yarn, and listening to other folks yarns. Hardest listener I ever run across. The story that made him famous—the

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one about jumping frogs he heard from a river pilot. Another good yarn he borrowed from Aunty Cord, his servant at the New York farm. Mr. Howells, the fella who run that magazine, the *Atlantic Monthly*, paid him most handsome and praised him something fierce for telling Aunty Cord's story. Mr. Twain, he were susceptible to flattery, specially from Mr. Howells. It's the soul-butter from Mr. Howells that got him up to communicating like plain folks does. After that he got up the piece on Jimmy, the black boy he met in Illinois for that big newspaper in New York. Nobody took much mind to Jimmy until Mrs. Fishkin and she thinks Jimmy gave inspiration to Mr. Twain in writing my yarns.

Truth be told Mr. Twain don't write like the widow Douglas cooks, each victual by itself. I reckon he found me in a barrel of odds and ends; things got mixed up, black and white, and the juice kind of swaps around, till nobody can't tell things apart.

Mrs. Fishkin, she says things go better this way. But here's the puzzler. She digs around that ole barrel with things all mashed up and squigily and aims to hand out all the black parts. But they been cooking a mighty long time now and those juices kind of soaked through everything and it's all looking kind of gray. And besides I couldn't no way understand sticking your hand in the barrel to begin with, most especially not a girl. Why I couldn't a done it.

Those professors, they do throw style into it, don't they? Mrs. Fishkin has a powerful appreciation of that style of Mr. Twain. She says he just knocked the spots out of some big-bugs name of Hemingway, Faulkner, and Ellison, turning me loose without putting on frills. But style is a slippery thing as I learned from Tom Sawyer. Where I seen doughnuts and jam and a busted up Sunday school picnic, he saw di'monds, and A-rabs, and elephants and things. The Concord Public Library Committee banned my book when it first come out, called it trashy and said the book was fit for a slum. They didn't see no di'monds and they warn't alone.

I see I was up a stump again, and got to have another think. I headed for the river, and shoved out from the willow cove where my raft was hid. And I drifted down the big still river, laying on my back and looking up at the sky, all speckled with stars. I took a swim now and then and got swallowed up in darkness. There warn't a sound there; the whole world was asleep, only the bull-frogs a-cluttering sometimes. Then it come to me. Some folks just can't hear the stillness.

There warn't no use in rattling on. If I'd a knowed what a trouble it was to make a review I wouldn't a tackled it and ain't agoing to no more. Tom Sawyer, an expert on all the best authorities, told me some Frenchman named Molyear said it's dad blamed hard to make decent people laugh. I reckon it's so cause people don't hardly object to being considered wicked, but they can't see no profit in appearing ridiculous.

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Reviews of Books

GENERAL

STEPHAN OTTO. *Rekonstruktion der Geschichte: Zur Kritik der historischen Vernunft*. Part 2, *Systematische Ausarbeitung*. (Die Geistesgeschichte und ihre Methoden, number 6.) Munich: Wilhelm Fink. 1992. Pp. 319. DM 88.

This volume aims to reinvigorate *Geistesgeschichte* (a historiography in which consciousness and its phenomena are central) by giving it the philosophical foundation that, according to Stephan Otto, it has lacked so far. For that endeavor, Otto's *Materialien zur Theorie der Geistesgeschichte* (1979) was a preparatory step. The author focuses on Wilhelm Dilthey's attempt at securing history as a *Geisteswissenschaft* without transcendent elements through a critique of historical reason (*Kritik der historischen Vernunft*) that could assure the validity of the historian's process of inquiry. Because of Dilthey's failure, *Geistesgeschichte* did not hold its own in the modern debate on the theory of history, even in Germany where it could rely on the long idealist tradition or at least on the intimate connection between philosophy and history. Otto maintains that *Geistesgeschichte*'s modern realizations—the history of ideas, intellectual history, and the history of mentalities—could not give *Geistesgeschichte* the authoritative voice in the modern debate because they have been flawed theoretically.

Otto traces Immanuel Kant, G. F. W. Hegel, Johann Fichte, and Dilthey's importance for a critique of historical reason (for him the transcendental-logical mastery of historical cognition) with special reference to the crucial connection between the individual historical phenomenon (*Einzelnes*) and the general (*Allgemeines*)—between the multitude of the historical world and its conceptual mastery. Dilthey short-circuited this connection when he placed both into the totality of life, submerging the cognitive act into that totality's smallest distinct unit, the *Erlebnis* (the discrete experience). Such a vitalistic holism (*lebensphilosophisches Ganzheitsdenken*) overlooked the fact that, despite the embeddedness of all thought in life, a proper critique of historical reason must start with the recognition of the categorical separateness of the individual and the general and with the need to keep them separate in the cognitive process. Otto has recourse to Hegel's *Schluss der Mitte*, where the individual and the general find their mediation in the concept (*Begriff*). When Dilthey saw historical phenomena as the direct expressions of life, in which the singular and general were merged, he excluded a true mediating activity by the historian because there no longer was a logical structure behind the phenomena. Hence, Dilthey's historian is a life-experiencing and life-submerged self (*Erlebnisselbst*) whose cognitive act is limited to describing phenomenological patterns of connections between life's segments (*Lebenszusammenhänge*). In contrast, Otto insists on the presence in a critique of historical reason of the subject as the cognitive "I" (*Denkich*) whose ontological standing enables it to keep a critical and constructive distance from the flow of life and history into which it is bound by its psychology and social ties. Such an "I" alone can create the area of reflection about history in which the general statement does not reduce the historical singular to an unimportant moment and in which, as Otto puts it, the subject can make a transcendental-epistemological contribution (*Verstehensleistung des Subjekts*).

Having ontologically secured the inquirer's person against any psychological or linguistic dissolution, Otto is equally insistent on differentiating clearly between the totality of experienced life and the totality of life rather than, as Dilthey did, equating them. He steers a middle course between the views that simply declare their theoretically or narratively mastered accounts to be the total reality and the static Greek and medieval ontologies that made the historical ephemeral. He relies on the conceptual pair of *Darstellung* and *Darstellbarkeit*. The first refers to that which is "given" to all cognition, not substantially (in the old sense of all of reality) but in its modalities (*modalontologisch*). Time and objectivity govern that which can be cognitively presented (the *Darstellbarkeit*). For Otto, modern, theoretically oriented, and narrative theories of history have shared in Dilthey's "deficit" by being victims of a double conflation, epistemologically (the reflective cognitive "I" with the life-experiencing "I") and structural-logically (the experienced totality with the true historical totality). Thus, Otto faults narrativists for seeing in the narrative the historical totality, *mentalité* theoreticians for

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accepting specific mentalities as the only valid horizons, textualists for giving texts that finality, and hermeneuticists for reducing the structure of thought to forms of expressions and being content with showing the order immanent in sensory experience. All of them presuppose but do not deal with the totality not yet cognitively mastered but real nevertheless.

Readers of the volume are presented with a sophisticated argument on important questions concerning the theory of history. English-speaking readers will find it an obstacle that Otto, striving to be precise, has used much translation-defying German philosophical terminology. He also could have made his stance clearer by extending his critical references and relatively brief discussions of such modern thinkers as Hans-Georg Gadamer, Jürgen Habermas, Theodor Adorno, Jacques Le Goff, Arthur Danto, Jacques Derrida, and Dominic LaCapra into sustained arguments. Still, Otto's argument on behalf of a *Geistesgeschichte* that spells out the philosophical foundations for historical theory should be of interest not only to Dilthey scholars, who will certainly scrutinize Otto's views on Dilthey's "deficit," but also to those interested in the theoretical issues in intellectual history, history of ideas, and the history of mentalities.

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ROGER S. GOTTLIEB. *Marxism 1844–1990: Origins, Betrayal, Rebirth.* (Revolutionary Thought/Radical Movements.) New York: Routledge. 1992. Pp. xvii, 248. Cloth $49.95, paper $15.95.

At this moment, it may seem odd to publish a new book on Marxism. In all its forms and currents, socialism as a political movement is, to say the least, in crisis and, if conservatives and liberals are to be believed, on its way to joining feudalism in the dustbin of history. Marxism itself, the central intellectual tradition of that manifold political movement, seems only to persist as the faith of selected Left splinter groups and as the theoretical perspective of "the last intellectuals," teachers in university humanities and social science departments. Nevertheless, no matter how many times we are told that we have reached "the end of history," there are those of us who look at history and contemporary life and feel compelled to continue to cultivate the Marxian intellectual tradition, that is, to refuse to give up both asking questions about exploitation and oppression and to contribute to popular movements seeking to confront and overcome them.

Clearly, Roger S. Gottlieb, an accomplished author and editor both on Marxism and on critical spirituality in the present age, is one of these people. Although I am disappointed with his new book, I welcome it as an effort to face up to the corruptions and tragedies of Marxism, to rescue the critical elements and possibilities to be found within it, and to pursue the dialogue between Marxian thought and other traditions such as feminism.

This study is not presented as a "history" of its subject but rather as a historical perspective on and critical redemption of the Marxist tradition. The subtitle of the work registers the volume's first three of four sections: origins, betrayal, rebirth. The two chapters of the opening section treat the work of Karl Marx himself, both the high points and the low, that is, both the brilliance (alienation/exploitation and class) and the basic flaws (positivism and determinism), as well as the omissions (gender, nationalism, imperialism) that have haunted Marxism's later development in the hands of lesser and more ambitious figures and parties.

In the second section of the book, Gottlieb considers and criticizes the two political paths of Marxism in the late nineteenth and early twentieth centuries: Social Democracy, which celebrated a positivistic and evolutionary reading of Marx and thereby reified history at the expense of agency and possibility, and Soviet Communism, which turned Marxism into Marxism-Leninism-Stalinism and used it to justify a brutal and horrific one-party state. He refers to the latter development as the "death of Marxism" but then proceeds to declare its "rebirth" in Western Marxism, with its attention to "consciousness," and Socialist Feminism, with its attention to "difference." Finally, in the three chapters of the fourth section, entitled "Present and Future," Gottlieb considers a few of the late twentieth-century contributions to Marxian thought such as state theory and world-systems theory, responds to some "radical critics" of Marxism such as sociologist Anthony Giddens, who developed "structuration theory" as an alternative to it, and argues that Marxism needs to be supplemented with "spiritual" values.

As a historian I was disappointed both by Gottlieb's superficial treatment of the thought of Antonio Gramsci, the most historically oriented of Marxist thinkers, and his failure to address the remarkable contributions that British and American historians have made to Marxist thought. In Gramsci's *Prison Notebooks and Letters* and the writings of E. P. Thompson and others, he would have discovered the critical theory and historical experience and agency so essential to the imagining and making of the values and social orders that he is so strongly committed to realizing. Once again a philosopher fails to adequately appreciate historical studies. I was also disappointed by his concluding chapter on Marxism and spirituality, which, however good and decent his intentions, reads too much like a New Age religious tract. Gottlieb makes good points about the failings of Marxist thinkers and explains difficult theoretical points crisply and clearly, but, because he fails to attend to the critical historiography that would have

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strengthened his arguments, I cannot recommend his book to my historian colleagues as much as I would have liked.

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STEPHEN ERIC BRONNER. *Moments of Decision: Political History and the Crises of Radicalism.* New York: Routledge. 1992. Pp. ix, 164. Cloth $49.95, paper $14.95.

This book suffers from a decision of the moment: that made by Stephen Eric Bronner when he chose to write an introduction promising more than the body of his book achieves. Bronner, a historically musical (as opposed to a historically tone-deaf) political scientist, successfully provides interesting, often informative, sketches of six "crises of radicalism" in the twentieth century. Without linking them in any depth, Bronner examines the decision by the parties of the Second International to participate in World War I; the decision by the Communist and Social Democratic parties of Germany not to unite to defeat Adolf Hitler in 1933; several key decisions by Léon Blum and his "Popular Front" government in France in the mid–1930s; several key decisions by Left parties in the Cold War years; and what Bronner sees as the main, self-destructive decisions by the New Left of the 1960s. The final chapter, a critical discussion of Francis Fukuyama's *The End of History and the Last Man* (1992), is meant to serve as a commentary on the present as a moment of crisis. A postscript discusses the failed coup attempt against Mikhail Gorbachev in the former Soviet Union.

Bronner expressly wants to retrieve and refurbish the tradition of social democracy, which he contrasts with Bolshevism and the immature extremisms of the New Left, as well as with what he sees as the disorientation and despair of the present. He places much emphasis on the need to situate the decision makers of radicalism in the context of the political constraints in which they worked. Although he is critical of specific decisions made by social democratic leaders, Bronner takes for granted the efficacy of the heritage as a whole (whatever that might be; it is invoked rather than articulated). In some respects, his study exemplifies what Nietzsche calls monumental history, with Karl Kautsky, Rosa Luxemburg, and, above all, Blum, being the monuments Bronner puts into the service of what Nietzsche called the life instincts, in this case, with Nietzsche's forbearance, those of social democracy. (Bronner also demonstrates the downside of monumental history: that the monuments erected can, by snuffing out the capacity to forget, turn against life.)

The selection of "moments of decision" is traditional, the conception of "political history" fairly narrow. Bronner, for example, writes as if feminist critiques of Marxism did not and do not exist. He makes barely a gesture toward issues of gender and race. I would like to have seen an appraisal of the moment it was decided to keep secret from the Marxist movement the existence of Frederick Demuth, the child born of Marx's extramarital union with the family's maid, Helene Demuth; a chapter perhaps on the decisions by the main currents on the Left earlier in this century to generate and circulate images of socialist and communist masculinity; or an appraisal of the decisions not to support concerns raised by André Gide and others regarding leftist discomforts with homosexuality.

The absence of focus on such matters plays havoc with Bronner's introductory claim that his book is "a contribution to the development of a political history from below" (p. 5). Instead we get such unexamined assertions as: "the profoundly humanist and democratic values of the labor movement become manifest in the positions taken by the Second International on the most important events leading up to the war" (p. 17).

What makes the introduction even more problematic, however, is that it indicates Bronner's keen disinterest in a political history from below. His book's focus is actually on the decisions of leaders on the Left. He may have a point when he asserts, without reference to any examples, that many recent approaches by social historians to working-class movements suffer from a "pointillism" and an "experientialism" that lose sight of structures, institutions, and politics in the details of workers' lived experiences. But his announced blend of "impasto and broad brush strokes" is not really an alternative. One cannot discuss the working class, he argues in an ironic reprise of the old Bolshevik impulse to substitute the worker's party and its leadership for the class itself, without focus on its organizations and their representatives. "And such organizations," he announces without qualification or hesitation, "do not merely perpetuate domination or suppress voices from below. They also crystallize the material and normative concerns of their constituencies" (p. 4). Bronner not only writes as if feminism never existed but also as if critics of institutions and power on the Left from Roberto Michels to Daniel Cohn-Bendit and Michel Foucault never did either.

In the breathtaking political whirl of the present, his opening page proclaims, history must be reckoned with "if working people and their most progressive representatives are to make good on the opportunities presented by this new period." The not uninstructive chapters that follow, however, suggest that at least one reasonably clear aspect of the present situation is that it has, finally, shattered any such easy use of the phrase "working people and their most progressive representatives."

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EBERHARD ZANGGER. *The Flood from Heaven: Deciphering the Atlantis Legend*. New York: William Morrow. 1992. Pp. xi, 256. $25.00.

"This book was not written to replace old dogmas with new ones, it was written to inspire the reader, to encourage the disregarding of conventional beliefs and to pursue new lines of thinking" (p. 222): thus Eberhard Zangger concludes his novel attempt to decipher the Atlantis myth of Plato's *Timaeus* and *Critias* as a historical but garbled version of Bronze Age Troy and the Trojan War.

To reach this conclusion, Zangger first accepts Plato's word that the tale is true without attempting any analysis of his use of myth; the account of Atlantis must be true simply because Plato says it is. After all, "if Atlantis really originated in Plato's mind only, he would have been able to produce a much more suitable story" (p. 43), although suitable for what is left unclear. Thus, Zangger believes that Solon did receive the tale of Atlantis from Egyptian priests, that he composed a confused version of the tale, and that this garbled manuscript came into Plato's hands. Unhappily, Plato not only misinterpreted these "notes" of Solon (p. 120) but he also had neither a sense of size nor a sense of chronology (p. 123).

These failings come in handy for disregarding those aspects of the Atlantis myth that are obviously not applicable to Bronze Age Troy, such as the size of the Atlantean plain or the date of Atlantis' existence. That Atlantis was an island and Troy was not shows the ignorance of the Egyptians; Atlantis being larger than Libya and Asia combined really reflects Troy's sphere of influence or its territorial claim (pp. 115–16); the Atlantic Ocean location is rejected in favor of the Black Sea, and a mirror-image map of the Mediterranean is printed to "clarify" the geography (p. 110). As for there being elephants on Atlantis, Zangger states (p. 141): "Elephants in Troy? Well, who can tell?"

More interesting is how Zangger came to consider Troy as the possible origin of the Atlantis myth. Chapter 1 recounts the author's analysis of an auger core taken at Tiryns; the presence of an unstratified mass of Late Bronze Age ceramics led him to postulate the occurrence of a flash flood, perhaps accompanied by earthquakes, which buried Tiryns' lower town about 1300 or 1200 B.C. Thus, there may have been floods and earthquakes in the Aegean around the time of the Trojan War—ergo, Atlantis. It should be noted that Zangger in a recent article ("In the Shadow of Mycenae," *Archaeology* [January/February 1993], 58) avoided mentioning any flash flood at Tiryns and spoke instead of soil erosion on a large scale, its cause unknown.

Having satisfied himself that Atlantis was Troy, Zangger attempts, in chapter 6, a unique interpretation of the *Odyssey*. Entitled "The *Odyssey* Starts Making Sense," the chapter proposes that Homer's Scheria is Troy/Atlantis and that Odysseus has traveled back in time to see the glory of Troy before the war. Thus, Demodocus sings of a war yet to come, so "the songs are meaningless to the Phaeacian lords" (p. 190); since Demodocus also sings of the wooden horse, one assumes that the bard, too, is a time-traveler.

Zangger even explains why the *Critias* was left unfinished by Plato. He abandoned the tale when he finally realized that what he was recording was a "freak version" of the Trojan War (pp. 43–45). The possibility that Plato just lost interest in the tale is dismissed as a "pathetic" theory (p. 43).

Chapter 8 concludes the book rather engagingly by listing the major shortcomings of the theory that Atlantis is Troy, Zangger even admitting that "when interpreting Plato I am willing to accept many of his assertions as correct; others however subjectively chosen, I totally reject" (p. 222). This candor is one of the saving graces of this book: no matter how unconvinced one may be by the author's arguments, it is impossible to assign him to the lunatic fringe of Atlantologists. Zangger, to be fair, has presented a theory as plausible as similar attempts to identify Atlantis with Crete, Thera, Spain, Great Britain, and other sites.

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ISAIAH BERLIN and RAMIN JAHANBEGLOO. *Conversations with Isaiah Berlin*. New York: Charles Scribner's Sons. 1991. Pp. xvii, 213. $20.00.

This book evokes many memories and also a certain regret. In the course of writing my doctoral dissertation, which Isaiah Berlin directed, I heard, and in a minor way participated in, numerous bigger and better "conversations with Berlin" than the ones found in this volume, and the same must be true of some of Berlin's other students and friends. Yet our talks remained unrecorded, whereas Ramin Jahanbegloo published an attractive volume of his conversations, clearly printed, almost without typographical errors, and with a fine photograph on the paper jacket and an index of names. For this he and Charles Scribner's Sons deserve praise.

What is the value of the book, and what caveats should be directed to its readers? To address the last point first, the volume is not to be taken as a summa of Berlin's thought and life, and perhaps not even as a good introduction to that thought. It is disjointed, fragmentary, of necessity peremptory in tone, and it moves too fast. Readers and students of Berlin will miss the amplitude and the nuance of the argument as well as much of the style. As Berlin wrote in the acknowledgments, "Mr. Jahanbegloo has kindly allowed me to read his typescript before it went to press, and as is usual in such cases, I wish to emphasize that responsibility for the precise form and cast of my words must lie with the author" (p. xii).

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The book, however, has important assets. Sketchy and in a sense rudimentary, it is also remarkably comprehensive. Even its five conversations, "From the Baltic to the Thames," "The Birth of Modern Politics," "Political Ideas: The Test of Time," "A Philosophy of Freedom," and "Personal Impressions" (devoted very largely to Russian intellectual history) with their total of fifty-three titled subdivisions do not fully reflect the richness and variety of the content. Everyone interested in Berlin and Berlin's thought will find something valuable here. There are also, of course, broader connections and implications. The account of Oxford, although sparse and narrowly focused, is just right in context, and it includes such items as a description of the movement known as "Oxford philosophy" (to call it an "insider's" account would be an understatement, because Berlin founded the movement in his college rooms in 1936, pp. 151–53). A tragic detail, related to the amazing general ignorance and neglect of the Holocaust among the Allies, is that Berlin, despite his connections and his wartime government service in Washington, London, and Moscow, learned of the Holocaust only very late ("Discovery of Auschwitz," pp. 19–23; Berlin lost two grandfathers, an uncle, an aunt, and three cousins to Nazi extermination).

The book refreshes and reinforces our understanding of Berlin's thought. Most importantly, it reminds us that Berlin has always remained primarily a philosopher. This is attested by the number of pages in this volume devoted to philosophers and philosophy, and even more by Berlin's approach to his material. Characteristically, Berlin's first objection to relativism is that "Relativism can't be stated, because the proposition which expounds relativism cannot itself be relative" (p. 107). And Berlin summarily dismisses numerous thinkers and schools of thought on the ground that he does not understand them, that is, that they are philosophically and logically meaningless. (Historians usually claim to understand everything, and we can only envy such absolute and instant dismissals.)

Yet although he is a sharp and severe judge, Berlin shines in this book as elsewhere as a thinker of remarkable humanity, empathy, and cosmopolitan aesthetic appreciation. "I remember a student who walked up to me outside the great mosque with the wonderful blue dome in Isfahan and asked 'Is there anything in England as great and as beautiful as this?' I thought, and in the end said 'I don't think so'" (p. 23).

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KARL F. MORRISON. *Understanding Conversion.* (The Page-Barbour Lectures.) Charlottesville: University Press of Virginia. 1992. Pp. xxvi, 246.

KARL F. MORRISON. *Conversion and Text: The Cases of Augustine of Hippo, Herman-Judah, and Constantine Tsatsos.* Charlottesville: University Press of Virginia. 1992. Pp. xx, 192.

These thoughtful and challenging books are studies in what Karl F. Morrison calls "cultural poetics." Despite many repetitions they are best read together, not as consecutive volumes of a single work but as complementary approaches to a common goal. They focus on accounts of the experience of religious conversion drawn from Western Christianity, specifically on what Morrison terms "empathetic conversion," that is, the inward, supernatural process by which the self is consciously altered to become more like the distantly apprehended and intensely desired Godhead. We are not concerned here with the means by which the Germanic tribes came to practice Christianity but rather the processes through which an Augustine of Hippo or a Bernard of Clairvaux came to view themselves as more fully Christian.

In *Understanding Conversion*, Morrison presents revisions of the Page-Barbour Lectures he delivered in 1990 at the University of Virginia. The first two chapters consider the hermeneutic problems posed by the experience of religious conversion. Morrison points out that the historian cannot approach such experience directly but must instead rely on texts which construct that experience, texts which often stress that the experience itself is ineffable. Countering the well-known thesis of Arthur Darby Nock (*Conversion* [1933]), Morrison argues that there is no single paradigm of conversion. Even the word "conversion" itself is merely a convenient metaphor for the experience, for it is not a univocal term but rather a "portmanteau" word into which various writers have packed varied meanings. The historian can only enter the hermeneutic circle by offering an interpretation of the interpretive process. In these opening chapters, Morrison makes three key points that structure the following argument in both books. First, all accounts of conversion are fictive, that is, "poetic" texts which attempt to mirror the experience through a process of mimesis. Second, being "poetic," conversion accounts work through a process of "empathy." It is a gradual process through which the self tries to become more like God. Finally, these accounts are written with "irony," or a self-conscious realization that the narratives themselves are fictive artifices, while at the same time exhibiting a deep understanding of the empathetic process involved.

In the next four chapters, Morrison proceeds to treat separate characteristics of the literature of conversion: the roles respectively of suffering, conscience, myth, and humility in these accounts. The individual conclusions Morrison reaches are too rich and variegated to be summarized here. He illustrates his arguments from a wide variety of Western texts, stretching in date from antiquity to the twentieth century; they also include many cross-cultural allu-

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sions. The greater the background brought by the reader, the greater the harvest to be gathered. The clear focus, however, is on Latin texts from the late eleventh and twelfth centuries, a period on which Morrison is an acknowledged master. References to Otto of Bamberg, Peter Damian, Guerric of Igny, Bernard of Clairvaux, and Hugh of St. Victor recur repeatedly. In this account the author develops a new version of the familiar—and to my mind often, as here, overstated—argument concerning the crucial role of that period in the development of Western notions of selfhood and identity. As Morrison puts it, "twelfth-century writers were the first since Antiquity to have a vocabulary with which to speak about the inner life with great refinement" (*Understanding Conversion*, p. 9).

In contrast to this discursive and thematic style, Morrison presents in *Conversion and Text* detailed exegetical studies of three specific texts. For Morrison this volume is the secondary of the two works, "providing documentation beyond what is supplied" in *Understanding Conversion* (p. xxii). Having presented a summary of that previous work in the form of an introduction, Morrison proceeds to argue from the specifics of the case studies to a similar set of conclusions. The texts in question are the well-known *Confessions* composed by Augustine of Hippo in the late fourth century; the *Short Account of His Own Conversion* by (or at least allegedly by) Judah, a twelfth-century Jew from Cologne, who took the name Herman at baptism; and the *Dialogues in a Monastery* by the Greek intellectual Constantine Tsatsos, who served as president of the Hellenic Republic in the 1970s. Morrison includes, conveniently for students, a translation of the work by Herman-Judah, otherwise available only in Latin. This book is something of a hodgepodge. Morrison never provides sufficient explanation as to why he has chosen to treat these specific, and quite varied, works together. The acknowledgments suggest the occasional nature of the genesis of the individual chapters; they fully cohere only in the light of reading *Understanding Conversion*. Based as it is on specific texts, however, this work on the whole remains more accessible to the general reader than does its companion.

The scope of the erudition exhibited in these works is truly impressive. The ambitious scale often leads to unexpected and brilliant observations, such as the location of the writings of Simone Weil and Edith Stein in a medieval tradition that links suffering to the problem of apostasy. Who else but Morrison would have thought to contrast Othloh of St. Emmeram and Herman of Cologne to these twentieth-century figures? Conversely, we also find Morrison exercising his jeweler's eye to great advantage in the detailed exegesis of specific passages, as in his musing about the impact that the reading of papyrus scrolls had on Augustine's imagery. But the very range of the material treated causes some problems. In particular, the cross-cultural comparisons, while often illuminating, are sometimes simply misleading. Repeatedly noting that there is no equivalent for the word "conversion" in Arabic, Morrison makes the rather extraordinary conclusion that "conversion was not institutionalized in Islam" (*Understanding Conversion*, p. 5), ignoring the concept and the practice of *jihad*. His discussions of Buddhism are confined to texts concerning the monastic path, primarily Zen; he fails to recognize even the complementary existence of the alternative, lay path in most Buddhist traditions.

Both books, impeccably produced by the University Press of Virginia, are written with an elegance not often encountered these days in historical writing. There are passages of real eloquence, as when Morrison discusses the often misunderstood role of suffering in Christian literature, a role necessitated by the function of desire in the process of conversion. At the same time, they are densely written works whose complex argument requires sustained effort on the part of the reader. Careful attention must be paid to the precise meanings that the author attaches to his terminology. Otherwise, statements such as "There was an inescapable disequilibrium between empathy and poetics" and "Empathy could never be entirely rendered into poetics" (*Understanding Conversion*, pp. 39, 62–63) will remain impenetrable.

In the end, these are books about the interior landscape of the human psyche. I must admit to feeling disappointed that Morrison's response to the theoretical problems posed by postmodernism is essentially to remove history into the realm of aesthetics. In doing so, however, he has laid down a provocative challenge to fellow historians, one that will force his readers to reconsider familiar texts, to ponder unfamiliar material, and, in the process, to be changed, which is, after all, in Morrison's own terms to be converted.

THOMAS HEAD
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DONALD HARMAN AKENSON. *God's People: Covenant and Land in South Africa, Israel, and Ulster*. Ithaca: Cornell University Press. 1992. Pp. xi, 404. $29.95.

This book jelled, Donald Harman Akenson explains, the day he saw the Voortrekker monument in Pretoria. He had met the mind that had built it before: in the fortified farmhouses in Ulster, where he had conducted his previous research, and in the military-agricultural encampments on the occupied West Bank of the Jordan. For him these structures—solid, granite-hard, no rough edges, meant to last—symbolize a special kind of society, based on the covenant theology of the Old Testament Hebrews. Although all of Western and indeed Islamic society has been strongly influenced by the ancient Hebrew seedbed, he argues, these three cases represent not mere imitation, but rather wholesale incorporation.

Moreover, although this theology was thousands of

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years old, although these peoples had long been under its influence, the appropriation took place rather quickly. At the crucial moments when Afrikaners, Ulster Scots, and Zionist Jews came to imagine themselves as nations (see Benedict Anderson, *Imagined Communities: Reflections on the Origin and Spread of Nationalism*, 2d ed. [1991]), needing desperately to distinguish themselves from the "Others" who surrounded and imperiled them, they found the message and metaphorical symbolism of the covenant between Yahweh and his chosen people peculiarly appropriate and took it over. For the Afrikaners, this formative period was the decades on either side of the Boer War of 1900–02; in Ulster, it occurred about the same time, when Home Rule seemed likely of enactment; in Israel, it came only in the 1920s and 1930s. These covenant societies, Akenson contends, are awesome things, based on a symbolic structure created before the Iron Age, but made and elaborated in the twentieth century.

Covenant theology, Akenson explains, works like computer software. You feed in your data and questions; it cranks out clear, unambiguous, if–then answers: who we are, what land is ours, how to keep ourselves pure and undefiled, how to relate to "Others" who are not. When a people with such a mind-set gains control of the power of a modern state and a modern industrial economy, quite extraordinary things happen. With utter seriousness, utter inflexibility, and utter lack of humor, the precepts of the covenant relationship are developed in their full complexity, taken to their logical conclusion. Anyone who has tried to deal with the statutes and cases making up what used to be called the Law of Apartheid will know precisely what he means.

The problematic for the book is therefore the Old Testament itself: understood seriously, taken literally. As always, the power of the biblical language and imagery speaks for itself. Although Akenson treats his three case studies separately, with only a few cross-comparisons, the central theme integrates them successfully. He has written extensively on Ulster before. Paradoxically, but not surprisingly, his coverage of Israel and South Africa seems somewhat fresher and more original. His analysis of South Africa, at least, is a sound and significant contribution. Although he is not the first to point out the importance of covenant theology in the ideologies of these societies, I learned something about its texture and its compelling force.

Like others who have written on these matters, Akenson is most persuasive in explaining how and why God's peoples are stubborn, inflexible, virtually incapable of reform, less convincing on why change can and does occur. After decades of a "by all means necessary" mentality, the situation in Northern Ireland has in fact stabilized, and the homicide rate is actually lower than that in most American cities. Although the outcome in South Africa is still in doubt, the Afrikaner ruling elite since 1989 has behaved in remarkably resilient, highly intelligent ways for which the history I have read (and written) has not adequately prepared me. Covenant peoples are awkward customers, Akenson concludes. Deal with them sensitively but firmly, give them room (but not too much room) to maneuver, let them have time to talk to themselves: in time even they may cast off the powerful but anachronistic symbolism of the Old Testament Hebrews and reenter something like a modern time zone.

Although I have no doubt about the importance of the covenant motif in these three societies, I am not entirely persuaded about their uniqueness. Are they really more inflexibly certain of their moral rectitude than, say, Bosnian Serbs, Shi'i ayatollahs, extremist Hindus, Sikhs, or the Daughters of the American Revolution? This is an admirably ambitious, absorbing, well-written piece of comparative history.

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DAVID N. LIVINGSTONE. *The Geographical Tradition: Episodes in the History of a Contested Enterprise*. Cambridge, Mass.: Blackwell. 1992. Pp. viii, 434. $19.95.

Historians, recognizing by now that all their writings are ideological in nature, might be tempted to assume that geography is a different kettle of fish. Surely, they might think, the description of the earth—*geography*—is a relatively objective activity? David N. Livingstone's aim is to disabuse historians of any such idea. Beginning with "Geography in the Age of Reconnaissance," his ten chapters engagingly review the ways in which geography has bent to the winds of change. His "geographical tradition" might be criticized as being rather anglophone in nature, but it would no doubt have been impossible in a single book to include all the European traditions with appropriate detail.

The medieval geographers, with their often fantastic tales of distant lands, gave way in the sixteenth century to relatively sober accounts of the newly found lands across the ocean; here, although Livingstone does not say so, the traditions of description inherited from Greece and Rome powerfully came into play, initiating what some call "new modes of colonial discourse." New mapping techniques were central to these developments and were also important in the new chorographical, or provincial, descriptions through which Europeans at this time began to map out the characteristics of their own regions, as they came more and more under the control of centralizing governments, anxious to know the extent of their territories. Sometimes these maps are involuntary testimony to shifts in the political balance of power, as in the case of those English county maps from which the royal arms are progressively effaced in new editions from the late sixteenth century onward.

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With the spread of reforming ideas, including the concept of the world as a "magnificent theater," geography took on a new importance, first exemplified in descriptions of the geography of the Holy Land, and then in vast enterprises like the *Cosmographia* of Sebastian Münster and the *Theatrum Orbis Terrarum* of Ortelius. These were relatively unfocused geographical ventures, but the increasing consolidation of the national states also led to works like those of John Speed and William Petty, geared toward encouraging schemes of national economic development. Livingstone might also have mentioned here the emergence of powerful new tools of cartography in France, where the idea of a base-map, pioneered by Nicolas Sanson in the mid-seventeenth century, allowed French ministers to visualize the country as a whole, with different political and economic data that could be imposed on it at will, thus leading to the possibility of much better formulated policies.

The eighteenth century saw the emergence of a deist geography, in which all the regions contributed to the operation of a vast and cunningly calculated machine. Here one sees the distant origins of our modern ecological geographies. Alexander von Humboldt added an altogether new precision to the measurement of interrelated geographical phenomena, and Thomas Jefferson used geography to advance the ideological interests of an expanding nation. During the nineteenth century, "geography was the science of imperialism *par excellence*" (p. 160), with the formation of the Royal Geographical Society in England and counterparts in most other countries.

In the late nineteenth century, geographers began to be concerned about identifying the "distinctive core" of their discipline, which seemed particularly to lie at "the meeting-point between the sciences of nature and of man" (p. 191). Livingstone spends his last three chapters on these problems of demarcation, which are of great interest to historians. He describes the rise and fall of "regionalism" and explains how "numerical language was adopted by practitioners lusting after scientific credibility" (p. 326); on the whole, these sections make depressing reading. The book is well written, if a little hasty in parts. No doubt, for instance, when he assures us that Humboldt was deeply committed "to humanitarian causes, like anti-Semitism and abolition," he meant to say something else (p. 136).

Historians who read this book are bound to be impressed by the travails of geographers, the more so bearing in mind the great influence that geographically minded historians such as Marc Bloch have had. In the end, Livingstone seems a little over-pessimistic in his conclusions. He has little to say about the marvelous possibilities for the human sciences of remote sensing, preeminently a geographer's tool, and he seems to play down geography's central role in the crucial ecological concerns of our time, which extend far back in history. The geographical tradition is surely less moribund than one might think from reading this book.

DAVID BUISSERET
Newberry Library

JOEL B. HAGEN. *An Entangled Bank: The Origins of Ecosystem Ecology*. New Brunswick, N.J.: Rutgers University Press. 1992. Pp. x, 245. Cloth $38.00, paper $16.00.

This is a history of ecosystem ecology from its roots in Darwinian biology to the "big science" projects of the 1970s. Although organized around the concepts and work of a few key figures, it is not an internalist history. Joel B. Hagen considers the scientists' working conditions, research funding and how it changed over time, the politics of science, and the gradual and incomplete shift from individual work to large-scale cooperative projects and continuing programs. The historian of science will find this a useful summary, a good analysis, and a fine addition to the literature. Environmental historians can consult it to their benefit, as it explains many of the ideas that (in one form or another) inform the public environmental debate. Historians who are simply interested in nature, nature study, or environmentalism will find it a good nontechnical introduction.

Hagen begins with Charles Darwin's entangled bank, where the chaos of competition hid the operation of nature's laws, and the legacy of Darwinian biology to the first generation of self-conscious ecologists, who began work in the early twentieth century. These ecologists provided ideas of biological community and its composition and bequeathed as well a creative tension between conceptions that stressed stability and order and those that emphasized the chaos of competition. In discussing the group that made ecology an academic discipline, Hagen concentrates on Henry C. Cowles and Frederick Clements and on the Clementsian ideas of succession to a climax state and the community as an organism. He dismisses as "historical myth" (p. 47) the conventional view of a ruling Clementsian orthodoxy, followed by a fall; Clements was never so dominant or his critics so few as that view asserts. He represents the next generation by Charles Elton and G. Evelyn Hutchinson, who added ideas of food chains and trophic levels, organizing communities around metabolism. There follows a lengthy analysis of the genesis, publication, and impact of Raymond Lindeman's paper (1942) on the trophic-dynamic aspect of ecology, which Hagen sees as "one of the great intellectual watersheds in the history of ecology" (p. 94).

The second half of Hagen's book deals with postwar developments. Here Hagen changes tactics. Eugene Odum and Howard Odum are the central characters, and their differing approaches and temperaments are the material for analysis of changes in ecological ideas and research; but the narrative, nec-

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essarily, lacks that apparently inexorable sweep of theory development. Not only are there more people and more theories but also it is not yet certain just which ones will be seen as significant. Some topics come round again in new form; Hagen reviews the modern development of evolutionary ecology. The situation here has also changed. Ecology had been a small field and its practitioners had worked with little equipment on problems they could often do alone. After the war it moved toward "Big Science," with its long-range projects, changing cast of researchers, and programs that required years and millions of dollars. Hagen's discussion of the alliance of ecology and health physics and the use of radioactive materials for ecological research will be of particular interest to those concerned with science and society.

This is an excellent place for anyone interested in the history of ecology to begin; it lays out the main lines of thought and places them in their social and intellectual context. Those already working on the history of ecology will find its signposts and markers useful reminders and spurs to thought.

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WOLFGANG SCHIVELBUSCH. *Tastes of Paradise: A Social History of Spices, Stimulants, and Intoxicants.* Translated by DAVID JACOBSON. New York: Pantheon. 1992. Pp. xiv, 236. $25.00.

In this book, first published in German in 1980, Wolfgang Schivelbusch argues that the emergence of the modern era in Europe was marked by the introduction of a succession of new spices, stimulants, and intoxicants, or *Genussmittel*, "articles of enjoyment" (p. xiii). Sequentially these were pepper, coffee, chocolate, tobacco, liquor, and opium. Each, he suggests, fit into a changing economic and social structure and met the needs of an emerging social class: coffee for a sober bourgeoisie, liquor for a proletariat seeking to escape horrible working conditions, opium encouraged by the British to make the Chinese docile and to help pay for the tea trade. Profusely illustrated, the book is rich in informative detail; beer, for example, was a source of nutrition in foods such as beer soup as late as the end of the eighteenth century in Germany (p. 23), and a map shows the incidence of coffee houses in a section of mid-eighteenth-century London (p. 56).

Schivelbusch's account, however, would be strengthened by more systematic attention to physiology and the history of medical theory. In discussing the spread of pepper as a luxury item, for example, he does not explain why the taste of pepper might have appealed. Medical theory was also more complex than indicated by the author. He argues, for example, that the brain was the part of the human body of most concern to the eighteenth-century bourgeoisie and that it alone was developed, cultivated, and cared for in the seventeenth and eighteenth centuries (p. 110). Eighteenth-century materialist arguments, however, also gave rise to medical theories such as those of Pierre-Jean Georges Cabanis, who held the stomach as basic to human behavior.

This study is an example of the popular German historical series genre and is described on the dust jacket as the last of a three-volume set by the author on "the origins of modern industrialized consciousness." The book has neither footnotes nor an index. Dubious statements include claims that salt has been a part of human civilization "since time immemorial" (p. 4), that "Arabic culture is dominated by abstraction more than any other culture in human history" (p. 17), and an anachronistic reference to the "Third World" during the period of British sale of opium to the Chinese (p. 223). Unsupported claims such as "in the nineteenth century industrialization brought such an intensification of social misery into workers' lives that the motive of escapism became far stronger than it had been in earlier times" (p. 149) hint of historical naiveté.

There are also contradictions, as in the claim that alcohol stimulates proletarian values of collectivity and solidarity (p. 166) but that industrialization introduced the phenomenon of the escapist solitary drunk as well. Cigarettes gained popularity, according to Schivelbusch, because they could be smoked quickly and therefore corresponded to industrial society in the nineteenth and early twentieth centuries, but he also notes that with the Crimean War cigarette use spread to the West from Russia, hardly an industrialized country at the time (p. 117). In a brief postscript, Schivelbusch, inspired perhaps by Perrier sales, suggests that water had come to be "celebrated as the height of gustatory delight" (p. 228), implying that substances need not be chemically intoxicating to arouse excitement or *Genuss*, which may also be induced by meditation, a Coca-Cola on a hot day, or the latest ice cream flavor. The omission of crack cocaine from the postscript also dates the book.

BERTRAM M. GORDON
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LOIS W. BANNER. *In Full Flower: Aging Women, Power, and Sexuality; A History.* (A Borzoi Book.) New York: Alfred A. Knopf. 1992. Pp. xi, 422. $25.00.

As the twentieth-century wave of feminism crests into and past the middle age of its proponents, we should expect ourselves to produce more scholarship on aging women. With this book, Lois W. Banner, who has played an important role in the institutionalization of women's history, begins just such a project, sending up a tantalizing flare on the subject of older women's relationships with younger men. Ranging widely over a vast number of sources from history and literature, and occasionally from anthropology, sociology, and psychology as well, Banner seeks to

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explain long-term trends in such age-disparate relationships and attempts to ground their "cyclic progression" in variations of patriarchal power structures.

Banner ranges over three thousand years, although the eras are unevenly covered. She begins with an assumed prepatriarchal world of powerful women and goddesses, "veiled" in the Homeric epics and in Greek mythology, who sport with younger men. They are ultimately killed, however, memorializing "the patriarchal overturning of goddess-centered relations" (p. 76). Banner continues through the misogynistic Roman world, which, except for Ovid, viewed sexualized aging women as foolish and dangerous. With a nod toward early Christianity, Banner sweeps on to the late medieval period, whose twelfth century she views as favorable for aging women of status because these women could consort with young nobility thanks to the phenomenon of courtly love and the absence of their men at distant Crusades. Even on lower social rungs, propertied widows could look forward to a merrier second marriage with younger aspirants.

But a shadow lengthens from the sixteenth century on: women's supposedly voracious sexuality, especially after menopause, could convert into dalliance with the devil and lead to witchcraft, for which thousands of mostly old and poor women were burned. At the same time, Banner argues, patriarchy remasculinized itself with erotic but martial male clothing that enhanced men's musculature and boasted a codpiece. This, she suggests, was indicative of a possible male proclivity toward bisexuality in early modern Europe as well as of the need for strong men in a period of nation-state formation. In the late seventeenth and early eighteenth century, some of the best and brightest older women fared well in the literary salons mentoring young men, but women were soon defeated by the new domestic ideal, according to which married women were "enclosed in heterosexual space" (p. 247).

The situation improved somewhat in the nineteenth century, Banner contends, thanks to the women's movement that valorized the reform work of older women, but their sexuality was buried under the new nurturing "grandmother persona." The witch had been neutered. The twentieth century brought youth culture, exaggerated by advertising's exploitation of sexuality. Material and ideological changes, however, including the second wave of feminism, counterbalanced that trend by creating more flexible life-cycle experiences in which women leave and reenter education, paid labor, and maternity. Finally, for inspiring models of wise and spirited aging, Banner looks to the heroic African-American women portrayed in novels by Toni Morrison, Alice Walker, and Zora Neale Hurston.

Banner muses over many kinds of sources, indicating an impressive and wide-ranging knowledge, but she fails to integrate that knowledge into a meaningful cultural history. She draws unabashedly on conflicting theorizations—now deconstructionist, now materialist, now psychological—to explain "phenomena" drawn indiscriminately from history or literature, leaving the reader breathless and confused.

What does remain clear is Banner's professed commitment to the notion of patriarchy, which posits gender conflict as central to human relations. Social class thus becomes a secondary category, seen to be falsely dividing women and keeping them powerless. In fact, by foregrounding older women, both historical and literary, whose privileged status and wealth gave them the most likely access to young lovers, Banner limits her inquiry to a narrow social band. She relegates the majority of less-privileged women to brief notices at the end of chapters. Moreover, this class-limited choice of subject encourages her to deemphasize major historical dynamics other than those of power relations between the sexes. Thus, we are left with essentialism after all, despite the author's disclaimer (p. 21). This result mutes Banner's inspirational intention of "telling an ethical tale" (p. 13), because ethics depends on choice, and her assumption of a relentlessly assertive patriarchy overpowers any liberatory possibilities.

In the end, we have only "'a meditation' on history, rather than a history per se" (p. 23), as Banner herself announces.

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Jan Nederveen Pieterse. *White on Black: Images of Africa and Blacks in Western Popular Culture.* New Haven: Yale University Press, with the cooperation of Cosmic Illusion Productions, Amsterdam. 1992. Pp. 259; 225 plates. $35.00.

Jan Nederveen Pieterse's unusual study originated as an extended discussion for a museum exhibition. The text, now translated from the Dutch by the author, was at first designed to accompany an exhibit of an ironically named "Negrophilia" collection at the Tropical Museum in Amsterdam in 1989–90 and then in Brussels in 1991. There are plans to show this marvelous assemblage of artifacts in other cities in Europe and North America.

The book is lavishly illustrated with hundreds of items from the exhibition, many in color. Although there are some paintings and prints dating prior to 1700, the great majority are from the late eighteenth, nineteenth, and twentieth centuries. As Pieterse describes this assemblage, "It consists of visual material—prints, drawings, illustrated magazines, books, comic books, posters, advertising material, packaging, decorative objects, statues, figurines, toys, and other items" (p. 15). His list is not inflated: the collage of pictures embraces such items as sheet music, scenes from films, postcards, cartoons, photographs, mu-

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seum tickets, cigar bands, beer mats, a bottle-opener, and numerous other ephemera.

Pieterse's focus is on the social, psychological, political, economic, and cultural functions that the "othering" of black Africans served white Western Europeans and North Americans. He emphasizes the ways the degrading of African peoples and their descendants in the United States served the needs of Western hegemony and exploitation. Keeping this central focus steadily in mind, his discussion ranges widely. It includes such matters as stereotyping and Western self-imaging, ideas about "race," savages, cannibalism, Christian missionary impulses, the curse of Ham, anthropology, sexuality, servants, entertainers, non-Negroid people who have been treated like blacks, as well as gender and class, social hierarchy, and cultural pluralism. Temporally, the text stretches from Aristotle and the Bible through the Middle Ages, the Enlightenment, Victorian colonialism, to modern decolonization of the Third World and its continuing reverberations.

Such a broad gauge gives a somewhat scatter-shot quality to the book as a whole. Indeed, Pieterse's introductory outline of the structure of the book—although intended as an aid to the reader—reflects the miscellaneousness that it attempts to alleviate. As an overview of an enormous topic, this study has both the virtues and defects of its genre. No single topic is explored in depth, but the reader gains a very broad picture.

While concentrating on the self-serving energy that has driven Western image-making of Africans, the book also has a wide geographical sweep. It steadily distinguishes the United States as a special case in contrast to Western Europe, owing to slavery and to the much larger number of Africans taken there. It is sensitive to the kind of cultural backwashing that could occur from a New World colony to its metropolitan base, as when a female black athlete from Surinam is pictured as a sports heroine in the Netherlands.

It also discusses interesting differences in the ways various West European peoples destructively "othered" Africans. These differences among European nations emerge rather obliquely; they would probably have received more thorough discussion if the artifacts on which the book is based were more evenly distributed. Primarily, these national cultures turn out to be British, French, and Dutch, as well as Belgian (because of Leopold's Congo) and German (because of late-nineteenth-century colonialism and because of much more recent events in Germany). There are items from Italy and even Switzerland, but very few from Scandinavia. Most conspicuously missing are Spain and Portugal, and this study does not make clear why this is the case. The reader is left wondering whether few similar artifacts were generated and/or preserved in those Iberian cultures or whether a dubious assumption is being made that race was not a major consideration for them.

In net effect, however, Pieterse's treatment of these degrading stereotypes as functions and weapons of Western colonialism is impressively persuasive. Many readers will also find it depressing and immediate: close to home and a story not yet at an end.

WINTHROP D. JORDAN
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BARBARA J. SHAPIRO. *"Beyond Reasonable Doubt" and "Probable Cause": Historical Perspectives on the Anglo-American Law of Evidence*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xv, 365. $42.50.

Sometime in a murky, distant, and perhaps partially mythical past, English jurors decided cases on the basis of their personal knowledge of events. But, according to Barbara J. Shapiro, by "the sixteenth century most of the personal knowledge had dropped away" (p. 1) and jurors became at least partially "gatherers and weighers of evidence" (p. 4) who not only heard evidence but also investigated crimes. By this time jurors "were listening to and assessing evidence introduced by private accusers and government officials" (p. 4). Initially jurors weighed the "plausibility of the [defendant's] denial and the demeanor of the accused" (p. 5), based on their personal knowledge of those testifying. But eventually jurors had to come to terms with the evidence presented by people they did not know, and this led to the problem of how one weighs evidence. What should be the "standard" of proof in a criminal or a civil trial? Shapiro's learned and thoughtful book examines the evolution of the standards of "probable cause" and "beyond a reasonable doubt" that we today apply to indictments and convictions.

One of the strengths of this book is Shapiro's discussion of how the legal system developed from asking jurors to determine guilt or innocence on the basis of a "satisfied conscience"—an essentially religious view of the world—to a standard of "proof beyond a reasonable doubt—a secular rational standard." Thus, the "medieval trial jury . . . survived into the modern world with a new, intellectually satisfying justification for its decision-making role in the criminal justice system" (p. 245). When circumstantial evidence and testimony replaced the jurors' personal knowledge of the crime and the concept of beyond a reasonable doubt "replaced God's knowledge" (p. 245), the modern jury and the modern trial had emerged.

This book leads to important questions about modern juries. For example, the early view that jury members should come from the same vicinage as the accused stemmed from the idea that jurors would be able to "know and evaluate facts independent of the judge" (p. 244) because they knew the parties to the case and might even know about the case. Today we want jurors who know as little as possible about the

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case and who have never met the parties, witnesses, attorneys, or the judge. We might conclude that the vicinage is no longer relevant. The recent trial of police officers in Los Angeles for violating Rodney King's civil rights illustrates, however, that the vicinage may matter for new reasons. It is now important that the jurors know the community.

Thus, as Judge Henry Friendly wrote in *U.S. v. Maybury* (1959), "The verdict of the [jury] . . . is the verdict of the *pays*, a 'country,' a neighborhood, a community." Judge Friendly noted that the jury serves as "'the voice of the country' against the possibly excessive zeal of prosecutors." Yet as the two recent Los Angeles trials proved, the "voice of the country" depends, to a great extent, on the vicinage of the jurors. Shapiro's detailed and sometimes dense book helps put all of this and similar problems into greater perspective. It is an important contribution to the growing literature on the origin and history of the jury.

PAUL FINKELMAN
Virginia Polytechnic Institute

ALISON GILBERT OLSON. *Making the Empire Work: London and American Interest Groups, 1690–1790*. Cambridge: Harvard University Press. 1992. Pp. xiii, 261. $39.95.

This volume by Alison Gilbert Olson addresses the classic question of why, given its modest coercive resources, the early modern British empire worked so well, more particularly why its burgeoning colonies for so long remained loyal to it and voluntarily complied with most of its directives. To earlier answers to this question, Olson adds "another explanation," specifically, the emergence of transatlantic interest groups—she refers to them as "the interests"—that provided colonists with "an informal but effective voice in the making of English decisions that affected them" (p. 1).

In support of her thesis the author here provides a general narrative of this development as it applied to the North American colonies from settlement through the American Revolution. Briefly recounting how during the seventeenth century associational interest groups slowly supplanted more traditional ascriptive (kinship) and institutional (church, trading companies) interest groups as instruments for supplying government with vital information and influencing government decisions, Olson describes the expansion and increasing effectiveness of associational groups—principally organized churches, mercantile networks, and non-English communities—during the six decades following the Glorious Revolution and these groups' subsequent decline in a changing political context after 1755. "Simultaneously filling a gap between the rapid growth of the colonial importance to the empire and the far more glacial growth of British knowledge about it" (p. 10), English interest groups, she argues, lobbied effectively on behalf of American colleagues throughout the first half of the eighteenth century. In a situation where most decisions were "confined to particular royal appointees considering particular issues affecting particular colonial regions" (p. 51), they traded information and electoral support for government concessions and thereby served "as a lubricant that helped to keep the British government stable and the empire running smoothly" (p. 11). Once colonial management shifted away from the crown and toward Parliament after 1755, however, these interest groups lost their effectiveness and forced the colonists to confront objectionable metropolitan measures on their own.

In this solid contribution, Olson brings much research and thought to bear on the study of the empire's workings. Her account of ways the interests functioned goes well beyond any earlier study, and her analysis of their organizational structure, the assumptions that underlay their successful operation, and the changing volume and sources of petitioning to the metropolitan government on behalf of Americans are especially valuable. Yet most readers will probably find her book frustratingly sketchy. Written at a general level, it offers no detailed portraits of the interests in operation and few specific discussions even of specific negotiations.

Whereas some readers may find Olson's modest claims for her findings admirable, others will regret her pointed resistance to considering the relative significance of the successful operation of the interests in comparison with other factors historians have adduced to explain colonial willingness to stay in the empire. She does not confront the important question of to what extent specific interest groups can be conflated with the larger colonial polities with which they were associated and into which the empire was organized. Particular interests may well have "cooperated with the British government because they were getting what they wanted out of it" (p. 1). But did their accomplishments, on behalf of themselves and clients, produce among the broader membership of colonial polities a secure conviction that they were "represented at the highest level of imperial decision making" (p. 124) and had some "influence over the way the empire was run" (p. 61)? Powerfully implicit in her analysis, such questions will require consideration before we will know how much significance to attach to the operation of the interests in explaining the success of the early modern British empire.

JACK P. GREENE
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ZEYNEP ÇELIK. *Displaying the Orient: Architecture of Islam at Nineteenth-Century World's Fairs*. (Comparative Studies of Muslim Societies, number 12.) Berkeley and Los Angeles: University of California Press. 1992. Pp. xv, 245. $40.00.

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The world's fairs of the late nineteenth century began with the Great Exhibition in London in 1851, but it was not until the Paris fair of 1867 that architecture from foreign lands was created on the site. This was continued at Vienna in 1873, Paris in 1889, Chicago in 1893, and Paris again in 1900. In each of these exhibitions there were displays of Islamic architecture and everyday life, typically consisting of an Ottoman quarter, an Egyptian quarter, possibly a small Iranian quarter, and, in the Paris exhibitions, Algerian and Tunisian areas. In these parts of the fair a pastiche of replica buildings representing the local architectural traditions was constructed and peopled with local inhabitants going about their "normal" daily lives.

These were no toy-town structures, and one has to be amazed at the energy, effort, and expense that our Victorian forebears put into what was only intended as a temporary exhibit, even if the Egyptian obelisks were sometimes made of wood and many of the street fronts were no more than façades. None of these exhibits survives in any physical form, but Zeynep Çelik has unearthed a mass of engravings and photographs showing what they looked like. As she makes clear, these exhibits gave a view of the Islamic world as picturesque and unchanging: an Orientalist view, if you like, of the East. If they were no more than picture-postcard caricatures, these stage sets would be a mere curiosity but, as Çelik suggests, they were more important than that. The patrons who sponsored them were Muslim governments in Istanbul, Cairo, and Teheran anxious to establish an identity on the world stage. To do so they had to look again at their own cultural traditions and define them to show how distinctive they were. Sometimes they chose real buildings as models—the mausoleum of Qayt Bay in Cairo seems to have been a favorite—but more usually these were composites to produce a "typical" Ottoman mosque or Tunisian market.

Çelik sees as one consequence of this a new concern for the use of traditional motifs and ideas in real buildings in Islamic countries, like the gate of the defense ministry (now the university) in Istanbul. It may also have contributed to a growing interest in and respect for the local vernacular architecture that bore fruit in Morocco with Marshall Lyautey's decision to respect the integrity of the old city at Fès and create an entirely separate *nouvelle ville*, or in Algiers with the use of Islamic motifs in public buildings. "The preparatory work for this phenomenon," writes Çelik, "had already been completed at the world's fairs where architects of the colonial pavilions had interpreted the Islamic architecture of the colonies according to Beaux-Arts principles" (p. 164).

Çelik seems uncertain how to regard this interest. By taking indigenous architecture seriously, it clearly encouraged a cultural self-awareness and self-confidence in a Muslim world increasingly buffeted by influences from the West and in danger of being totally submerged. Yet much of the comment from Western sources was condescending, and the whole conception was designed to make the Muslim world seem quaint and conservative in contrast to the dynamism of the West.

In discussing such issues, Çelik is in danger of investing these rather flimsy structures with more theoretical weight than they can safely bear by neglecting the Western context of these fairs. They were meant to display and above all entertain. They were, so to speak, a sort of reverse tourism where, instead of people from the West visiting Muslim lands, the Muslim lands came to them. In an era when many fewer people were able to travel, and when no one saw foreign countries on film or television, these displays had a real novelty; what sometimes looks to us like kitsch on a grand scale was at that time a real revelation. To be successful, the fair had to entertain the home audience and so had to make exhibits from other lands curious and interesting, just as the modern tourist seeks out the old quarters of Damascus rather than the new suburbs.

The late nineteenth century was also the high point of the arts and crafts movement in Britain and similar movements elsewhere. There was a universal concern to preserve and even return to traditional forms and craftsmanship. In three fascinating illustrations (pp. 54–55), Çelik shows us Old Vienna from the Chicago exhibition of 1893 and Old Paris from Paris 1900, both absurdly quaint and fantastic; it was not just the Islamic world in which the unchanging and traditional was sought out. The author has found a fascinating corner of architectural history and illuminates it with thought-provoking skill.

HUGH KENNEDY
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JOHN L. OFFNER. *An Unwanted War: The Diplomacy of the United States and Spain over Cuba, 1895–1898.* Chapel Hill: University of North Carolina Press. 1992. Pp. xii, 306. Cloth $39.95, paper $14.95.

In this long-awaited study of the diplomacy surrounding the Spanish American War, John L. Offner maintains that the conflict was "inevitable," primarily because the differences between Spain and its Cuban colony were irreconcilable. None of the major players in this three-sided drama wanted war, but in the end it was the Cubans who were unyielding, inflexible, and hence responsible for bringing on the conflict. Although President William McKinley, who, according to Offner, "dominated American foreign affairs," sought peace, it became necessary for the United States to intervene in order for the Republicans to win the Congressional elections of 1898 and "keep control of Washington" (p. ix). Far less important in Offner's view were the issues that historians traditionally have emphasized in explaining these events: expansionism, the search for markets and investment, the sensational press, and national security interests.

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This volume is the culmination of a life's work of research, study, and scholarship. For more than a quarter of a century Offner patiently and exhaustively combed the archives of this hemisphere and Europe. His multiarchival research, which is grounded in extensive work in U.S. sources, has taken him to collections in Spain, England, France, Austria, the Vatican, Cuba, Brazil, Mexico, and Argentina. As a research effort this monograph is definitive. But disappointingly, for all his effort and time spent in foreign archives and personal collections, Offner has not appreciably altered our basic knowledge of U.S. diplomacy or that of the Latin American nations. He has, however, broadened and enhanced our understanding of the thinking of Spanish military and political leaders and "the inner workings of the Spanish government." Thus, "armed with a richer understanding of Spanish politics and diplomacy," we do gain "a better understanding of the diplomacy of the McKinley administration" (p. x). This is true as well for the diplomatic positions of the French, Austrians, and British, all of which Offner thoroughly discusses.

In addition to his opening statement that the conflict between Spain and the United States over Cuba was inevitable, which strikes me as a strange line of argument for a historian to take, Offner's other main interpretive theme is that Congressional Republicans "made war on Spain not to obtain control of Cuba but to retain control of Washington" (p. 234). It may be axiomatic that politicians seek reelection as surely as night follows day, but in this instance the author's contention about the political motivations of Republican legislators remains largely unproven. Curiously, given the exhaustive nature of his research, Offner provides little hard evidence in the form of roll-call analysis, direct statements (either public or private) from GOP leaders in the House or Senate, or other proof that victory in the fall election campaign was uppermost in the minds of those Republicans who voted for war with Spain in April 1898.

Equally curious, given his clear dedication to interpretive balance, fairness, and comprehensive treatment, is Offner's reluctance to incorporate in his analysis the mainstream scholarship the field has generated over the past thirty years. He ignores the implications of the work of Michael Hunt on ideology and race, Tom Terrill on the tariff, and the comprehensive analyses of this era presented by William Appleman Williams and Walter LaFeber. Also, in contending that the war was brought on primarily because of the intransigence of the Cuban revolutionaries, Offner does not directly confront the work of Louis Pérez, nor does he refute Pérez's contention that the Cubans were poised to win independence on their own in the summer of 1898. Offner does acknowledge, however, the arrogance of the McKinley administration's belief that the Cubans needed American tutelage by noting that Washington "confidently assumed the burden of aiding and guiding reluctant subjects, believing it could do a better job than Spain" (p. 236).

Offner criticizes McKinley's inconsistency in urging the Cubans not to settle with Spain unless satisfied, while at the same time expecting them to unquestioningly accept whatever the United States offered. Despite this criticism of McKinley's insensitivity in dealing with the Cubans, Offner's overall appraisal enhances the Ohioan's reputation as a strong leader who was in charge of the nation's foreign policy. On this point, this book is in accord with recent scholarship on the McKinley presidency and supports the earlier work of Margaret Leech, H. Wayne Morgan, and Lewis Gould, all of whom have sought to rescue McKinley from the stigma of Theodore Roosevelt's canard that he had the spine of a chocolate éclair.

EDWARD P. CRAPOL
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CHRISTINE A. WHITE. *British and American Commercial Relations with Soviet Russia, 1918–1924*. Chapel Hill: University of North Carolina Press. 1992. Pp. xii, 345. $39.95.

Christine A. White argues that both Britain and the United States engaged in extensive indirect trade with Soviet Russia in the years immediately after the October Revolution. In her view, American and British trade with the Soviets between 1918 and 1924 was "not insignificant." Beyond this, the author contends that an increase in direct trade between the Soviet Union and the West at the end of the 1920s was largely based on a foundation of experience and trust established in the first part of that decade. According to this interpretation, the Soviets' ability to meet all financial obligations, in spite of extremely difficult economic conditions, convinced Western government and business figures alike that they were reliable commercial partners.

White's study is well written, but her principal conclusions are hardly original and are often contradictory and unsubstantiated. She often seems uncertain of her own position on the issues. Her treatment of British trade with the Soviets is much more thorough than her depiction of Soviet-American commercial relations. Although the author did extensive research in Western Europe and the United States, she failed to research the topic in what were then Soviet archives. She is mistaken in assuming that these Soviet materials would have been unavailable.

White stresses that in the early 1920s British government and business representatives perceived that more normal Anglo-Soviet relations would have a positive impact on the domestic economy. But at the same time the author admits that the volume and value of Anglo-Soviet trade stayed well below prewar levels throughout the decade; and American exports to Soviet Russia attained the prewar percentage of total American exports only by the middle of this

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period. To be sure, individual companies and specific sectors of the domestic economy benefited, but the common perception of Soviet Russia as El Dorado was never actually realized.

White's analysis is much clearer when she deals with the motives for Allied intervention in the Russian Civil War and the Anglo-American commercial rivalry in Soviet Russia. As she understands the situation, the intervention in the Russian north and Transcaucasia was essentially the result of British efforts to protect and extend their economic interests. In this interpretation, the British occupation of the Russian north was more imperialistic than the Allied interventions in other areas. The Americans and the British intensely competed for control of the oil fields in the Russian south.

White correctly points out that there was no unified Allied policy toward Soviet Russia. Britain and the United States disagreed over commercial, not political, matters. Just as important, as the author convincingly demonstrates, interdepartmental frictions in both the American and British governments prevented the implementation of any coherent economic policy toward Soviet Russia.

Specialists will be disappointed by portions of White's book, but they will particularly benefit from her insights into the Anglo-American commercial rivalry in Soviet Russia. In spite of its flaws, this is a solid piece of historical scholarship. The book reads well and is almost free of technical errors.

TIMOTHY E. O'CONNOR
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FRANK COSTIGLIOLA. *France and the United States: The Cold Alliance since World War II.* (Twayne's International History Series.) New York: Twayne of G. K. Hall. 1992. Pp. xvii, 316. Cloth $27.95, paper $13.95.

The Franco-American relationship, which began so auspiciously in the eighteenth century, has in the modern era been characterized largely by instability, cross-purposes, and cultural tension. Frank Costigliola's study brings out the various clashes of interest since 1940. The diplomatic history is supported, however, by some perceptive cultural and psychological analysis that, connected persuasively as it is to the episodical turbulence of everyday politics, makes this book unusually appealing.

The political story contains no major surprises. Costigliola leads us authoritatively through the various stages of the acrimonious wartime relationship between President Franklin D. Roosevelt and General Charles de Gaulle. The Cold War, with its dual threat of Soviet aggression and communist subversion, together with a persistent fear of Germany and acute economic distress, all pushed the politicians of the Fourth Republic into the American embrace. But it was an unwilling subjection. Resentment over what was seen as American manipulation and betrayal in Indochina in 1954, and then again in the Suez crisis of 1956, was amplified when de Gaulle returned to power in 1958. This event ushered in a much more assertive French diplomacy. The intensified emphasis on an independent nuclear force, the withdrawal from NATO's military structure, and the veto of Britain's entry into the Common Market presented a series of challenges to American policy. All this is admirably conveyed, although the complicating role of Britain itself perhaps deserves more attention.

De Gaulle, of course, was bent on a renewal of French eminence that could only come at American expense. The fundamental flaw in his system, as Costigliola shows, was his inability to find the necessary partner. The Germans, unsurprisingly, were enthusiastic about economic integration. For security they stubbornly looked to the United States, as indeed de Gaulle did in moments of real danger such as the Cuban Missile Crisis of 1962. De Gaulle occasionally courted the Soviets too, but they also preferred to deal with Washington over serious issues. The unilateralist Gaullist approach has nevertheless persisted since 1969, even under the socialist President François Mitterand. But realism constantly intrudes. Fear of Soviet missiles in the early 1980s and of German reunification in 1989 quickly produced a noticeably warmer respect for the American connection.

Costigliola emphasizes the influence of personalities and perceptions. Rooseveltian arrogance (unpersuasively linked to President Woodrow Wilson) and Gaullist pretentiousness cast a long shadow forward. American leaders have since seen themselves as essentially reasonable men consigned by fate to dealings with the flighty, "feminine" French. The French have complained in turn of an "Americanization" that involves both ruthless economic penetration and the permeating blight of a vulgar popular culture. Instead of harmony in the alliance, therefore, there has been a constant state of intrigue, impatience, and irritation.

Not that the Americans have won all the cultural battles. New York captured the limelight in modern art in the 1940s. But the successful American in Paris, from Gene Kelly to Sylvester Stallone, has typically been an entertainer. French influence here has been rather more profound. Jean Paul Sartre, Michel Foucault, and Jacques Derrida are just a few of the subtle French determinists who have, with varying success, tried to subvert the conventionality of American intellectual life.

It is a tribute to Costigliola's success that one is left wishing for more. Jean-Luc Godard and his imitators, and the African-American writers' community in Paris seem to deserve a mention. And what of the Protestant-Catholic dichotomy, not to mention the venomous debates over free trade? Despite these omissions, this excellent book deserves a wide readership.

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LILIANA SAIU. *The Great Powers and Rumania, 1944–1946: A Study of the Early Cold War Era.* (East European Monographs, number 335.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1992. Pp. xiii, 290. $36.50.

This work is a straightforward presentation of Romania as an issue and concern in the diplomatic relations and calculations of the United States, Great Britain, and the Soviet Union from the ouster of the regime of Ion Antonescu in August 1944 to the making of the Romanian peace treaty at the end of 1946 (the treaty was actually signed in February of 1947). Liliana Saiu traces the evolution of Romania from Axis satellite to active participant in the final defeat of Nazi Germany to Soviet satellite through a careful analysis of interallied relations.

On the whole, Saiu is successful in describing this process. She demonstrates how Great Power policies toward Romania were affected by contemporary events (military, political, and domestic) as well as by ideology, principle, and personalities (especially that of U.S. Secretary of State James Byrnes); effectively traces the unfolding of these policies; and clearly shows why Romanian issues were usually (and logically) subordinated to other considerations (the conduct of the war, the Italian and Japanese armistices, the establishment of the United Nations, for example).

Saiu's effort to determine "up to what point the end result was inescapable" is somewhat less focused, partly because, as it emerges, the possibility of a non-Sovietized, postwar Romania appears to have been virtually nil. The relevance of the Romanian case to the Cold War is explained but given less space than might be desirable. (Saiu notes a number of mistakes by revisionist historians whose lack of substantive knowledge about southeast Europe often vitiates their work.) The crucial role of the Allied Control Commission is given appropriate stress, but a more comprehensive discussion would have been helpful. Finally, the treatment of the Romanian peace treaty is a bit sketchy.

The materials used for this study are wide ranging and appropriate, with a few caveats. Because of the abundance of materials on the Anglo-American side and the paucity of materials on the Soviet side, the treatment is slightly skewed to the former, but this is unavoidable. The papers of Burton Berry (principal American representative in Bucharest) and an interview with King Mihai make significant contributions.

Saiu does not, however, draw enough on Romanian sources apart from the fragmentary archives now available. A good deal of what was published prior to 1989 was tendentious, but some important works are overlooked, such as the memoirs of the first Romanian ambassador to the Soviet Union, Iorgu Iordan (identified in the text as "Jorgu Jordan") and studies on Romanian diplomacy from 1944 to 1947 by Gh. Buzatu, Vasile Dobrinescu, Eliza Campus, and Ion Enescu, and on the Romanian peace treaty by Campus, Şt. Lache, and Gh. Ţuţui. A second deficiency is Saiu's overreliance on diplomatic reports to deal with internal Romanian developments. Despite the utility of such reports, they do not provide a comprehensive enough picture.

Overall, this is a useful work. It avoids the "heroes and villains" approach, it helps us understand an important segment of Romanian and diplomatic history, and it does this with balance, perspective, and accuracy.

PAUL E. MICHELSON
Huntington College

ANCIENT

FRANÇOISE AUDOUZE and OLIVIER BÜCHSENSCHÜTZ. *Towns, Villages and Countryside of Celtic Europe.* Translated by HENRY CLEERE. Bloomington: Indiana University Press. 1992. Pp. 256. $49.95.

In introducing the theme of their book, Françoise Audouze and Olivier Büchsenschütz quote a comment by Fernand Braudel that suggests that the rural landscape of France, dotted with villages, hamlets, bourgs, and isolated farms, is really an ancient creation. Its history runs back into time, before written records, long before the ancient Greeks and Romans entered the country. Understanding how traditional agrarian Europe was formed is the province of archaeology, hence this excellent survey of the pre and protohistoric periods (the latter term meaning records contemporary with written sources) in northern and western Europe.

This well-illustrated volume summarizes a large body of material, primary and secondary, from which prehistorians reconstruct the formation of rustic Europe (in professional parlance, settlement archaeology). Beneath the survey lie several important assumptions that historians of preindustrial Europe should take seriously. One is that, in terms of population, land use, and human activity, the rural sector of Europe was dominant until recently. Fieldwork has shown the enormous antiquity of the transformation of the European countryside, dating at least to the Bronze Age some four thousand years ago, and how close premodern farming areas and villages were to their predecessors.

A second theme, in line with a general trend among scholars, is to see the development of prehistoric Europe as indigenous. For too long students of early Europe have viewed ultramontane regions from a Mediterranean perspective. One still reads general histories of German village life based on the secondhand accounts of Tacitus and of Gallic (Celtic) communities—including proto-urban *oppida*—as if Caesar were the sole authority to which archaeological finds must be matched. More than a half century of fieldwork has made clear that these sources are at best

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misleading. "Barbarian" Europe held a variety of social and economic forms. Nevertheless, according to the authors, most of Western Europe was one autochthonous civilization by the late Bronze Age/Iron Age (circa 1000 B.C.E.), the product of long internal development.

After good, brief introductions to the history of prehistoric studies and research methods, the central chapters in the book are devoted to such matters as settlement patterns, field types, house architecture, fortification construction, and the details of building methods (including carpentry techniques such as the Bronze Age innovation, the log cabin). Later chapters on social organization and the development of true villages and towns in the Iron Age are particularly interesting. Sprinkled throughout the book are many sharp observations about interpretations of the periods in question.

One warning to historians: although very good about simplifying the myriad culture names used in archaeology, the authors make many assumptions about the level of readers' knowledge of the field. Also, readers should turn to the short discussion of climate change at the beginning of chapter 9 to put the whole period into proper environmental perspective. This is a long-needed summary of continental later prehistory, as good an argument as there is for linking archaeology with more conventional history.

BRUCE KRAIG
Roosevelt University

MOGENS HERMAN HANSEN. *The Athenian Democracy in the Age of Demosthenes: Structure, Principles and Ideology*. Translated by J. A. CROOK. (The Ancient World.) Cambridge, Mass.: Blackwell. 1991. Pp. xvi, 410. Cloth $69.95, paper $24.95.

Mogens Herman Hansen is one of the foremost scholars of Athenian democracy. This description of democracy in classical Athens, "intended not for specialists but for students" (p. ix), draws on about seventy books and articles by Hansen. Thus, there are no surprises here, nor can there be any question that this is a valuable treatment of a fundamental topic in the history of Western civilization.

Hansen begins with two chapters to help students of subjects other than ancient Greece and Rome. He includes a helpful discussion of the types of evidence that have survived from the ancient world and of what sorts of information these sources provide. Chapter 3 is an account of Athenian government from its beginnings to the end of the fifth century.

Chapter 4 is a general introductory consideration of Athens and democracy. The bulk of the book is a detailed description of the social structure of Athens and the institutions of government. A final chapter summarizes the nature of democracy in Athens. There follow maps and plans, a bibliography, an extensive glossary, and indexes. J. A. Crook's translation is smooth and clear. There are typographical errors throughout, but only one that might confuse students: Didymos has been placed in the fifth century B.C., rather than the first (p. 17).

The historical summary in chapter 3 is presented with such little comment that the principles of selection are not always clear. When Hansen does reflect more generally in chapter 4, the characteristic limitation of his work becomes more evident. Hansen's interest in the institutions of government has always been part of a rather compartmentalized view of society and culture. Seldom does he consider the ways that the institutions fit in with other aspects of social life. Thus, when he criticizes Josiah Ober (p. 63) for comparing the "audiences" of the assembly and of the dramatic festivals, he is technically accurate, but he has missed the more important ways in which these institutions overlapped and functioned in Athens.

Hansen is at his best describing the details of the composition of social groups and the workings of institutions. Characteristically excellent is his description of the relative status of citizen, metic, and slave (pp. 86–88). He is correct to emphasize the importance of the subdivision of citizens into those older and younger than thirty and the effect this must have had in some areas of government (pp. 89–90). Other outstanding discussions are those of the course of events in assembly meetings (p. 146), of the number and nature of decrees passed by the council (pp. 155–57), and of the evidence for "party politics" (chap. 11). To these topics and others Hansen brings a wealth of statistics and concrete detail that result in vivid and persuasive descriptions of the structure and function of government. In this area he is perhaps unsurpassed.

Because this book is meant as an introduction, Hansen has kept discussion of scholarly controversies to a minimum. Still, Hansen sometimes exaggerates the difference between his views and those of other scholars. For example, on the question of the "sovereignty" of the assembly in the fourth century (pp. 154–55), the various academic positions are simply different ways to describe basically the same practical phenomena. Sometimes, when Hansen uncharacteristically ventures into interpretation, the reasoning is difficult to follow. He claims, for instance, that the reuse of jurors' tickets by various men shows that there was competition for places on the jurors' board and that there was interest in this aspect of government (p. 182). Such competition and interest may have existed, but recycled tickets are not evidence for them. Mortality among jurors meant inevitable turnover, and pay may have been more a motivation than civic interest.

All in all, this is an admirable book, perhaps the fullest and most vivid account of the most important example of participatory democracy known.

RICHARD GARNER
Yale University

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HANS VAN WEES. *Status Warriors: War, Violence and Society in Homer and History*. (Dutch Monographs on Ancient History and Archaeology, number 9.) Amsterdam: J. C. Gieben. 1992. Pp. 455. f. 130.

Ever since the publication of Friedrich August Wolf's *Prolegomena ad Homerum* (1795), scholars have debated the "Homeric Question." Subsequent discoveries have contributed to the debate: Heinrich Schliemann's excavations at Troy and Mycenae in the 1870s, which demonstrated in the archaeological record the existence of a sophisticated Bronze Age civilization, and M. G. F. Ventris and John Chadwick's deciphering in 1953 of Linear B script as an early form of the Greek language. These advances demonstrated the historicity of a powerful Greek civilization between 1600 and 1100 B.C., to which the Homeric poems, the *Iliad* and the *Odyssey*, apparently made reference. In the last forty years, one important aspect of the Homeric Question has been to determine the extent to which the Homeric epics accurately reflect Greek society and the exact period of history they reflect. Hans Van Wees now offers us a provocative volume whose purposes are to confront the problems of using Homer as a historical source; to offer a partially new interpretation and to defend some old views against recent interpretations; in short, to shed some light on early Greek history. This he accomplishes by a detailed analysis of the epic materials, amply documenting his own views and interpretations by quotations and discussion of relevant portions of the *Iliad* and the *Odyssey*.

The focus of the study is on war and conflict in Homeric society, and on the causes of violent conflict between individuals and between communities. The general question, "Why do men fight?", is therefore also an object of this study. Van Wees asks whether violence is natural, restrained only by the social contract, the growth of society, the state, and laws, as seems to be the common view. His novel conclusion is that, on the contrary, violence is part of the very fabric of the society that the epics reveal and that it serves a crucial purpose in helping to determine the status of the warriors of the Homeric world. His first chapter sets out rules for investigating the epic world, which he argues may be, to some degree, "fantastic," but which is also consistent and not a random patchwork quilt of elements from different historical periods, pace prevailing scholarly opinion. Subsequent chapters attempt to reconstruct aspects of the epic world and then to compare the picture to archaeological and historical evidence. The results are interesting and often novel, but they will not convince every reader. In particular, those unfamiliar with the Homeric epics will experience some difficulty in assessing the argument.

Homeric society, in Van Wees's view, is polarized around power and powerlessness, honor and shame, force and violence, and submissive dependence. It is a world of hierarchical status, in which the worth of an individual is the product of power, strength, wealth, esteem, deference, and the like, and in which anger and revenge are the dominant motives of conduct. Conflict in this world results from struggles over status, which are often, but not necessarily always, resolved by conflict, violence, and bloodshed. Just as individual kings, chieftains, and warriors strive to achieve status by imposing their power and their will on others, so also communities seek status in relation to others by similar means. Warfare and violence, therefore, are both endemic and essential.

Van Wees informs his interpretation by anthropological and sociological theory, but he is also critical of the models brought by other scholars to this same epic material (for example, M. I. Finley in *The World of Odysseus* [1954]). He rejects Finley's models of the Homeric economy and the role of gift-giving in it, and he argues that the acquisition of wealth in its own right was a prime objective for Homeric warriors, in contrast to the view that the Homeric economy was a subsistence one in which the self-sufficiency of the community was the ideal. He also rejects the idea of a chivalrous code governing the fighting among the heroes, insisting instead that there is ample evidence that the heroes were much more interested in results than means and that they would "play dirty" if need be to achieve their goal of victory with the resultant increase in status. Among other arguable conclusions certain to stir scholarly debate are the suggestions that cities were the political units of the Homeric world; that the Homeric Greeks had a collective sense of self, denoted by the term Pan-Akhaians; that the epic world reflects the historical period of the eighth and early seventh centuries; and that many aspects of this world of violence continued down into classical Greek times as cultural constructs.

It is regrettable that a book published in 1992, purporting to deal with the heroic world of the Greek epic, and especially with the place of violence in that world (compare this book with S. Weil's *The Poem of Force* [1939]), fails to give adequate discussion to the epic view of women, especially as prizes of war. One thinks of Chryseis and Bryseis in Book I of the *Iliad*, or the unnamed women offered as prizes in Achilles's Funeral Games for Patroclus; were they not prime objects of this violence? If, as Van Wees suggests, much of the ethos of the heroic world carried down into archaic and classical Greek times, we should note the origins of patriarchy and misogyny in this world view as well. After all, society is not exclusively male, even if the Homeric heroes were.

CHARLES D. HAMILTON
San Diego State University

TIM G. PARKIN. *Demography and Roman Society*. (Ancient Society and History.) Baltimore: Johns Hopkins University Press. 1992. Pp. xvi, 225. $29.95.

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Tim G. Parkin's avowed purpose in this book is to "introduce ancient historians . . . to the study of the population structure and dynamics of ancient Roman society" (p. xi). In this he does an admirable job. Conversant with the craft of demography and familiar with ancient Rome, Parkin provides an ideal corrective to the sometimes demographically naive social histories of Roman society that have appeared recently. To be sure, in terms of demographic methods, this book says nothing that the interested student cannot get out of standard demography textbooks or one of the numerous manuals on demography for historians. Nonetheless, since Parkin employs Roman examples to make his point, his work is more likely to be read by ancient historians. This work serves a valuable purpose.

The book is divided into three sections. In the first, Parkin discusses the sources for Roman demographic reconstruction. Epigraphical, textual, and archaeological sources all come under his critical eye, and he finds each deficient in some way. But he transcends the tendency toward undue gloominess regarding ancient sources and shows that, if used with care, much can be done with the evidential base preserved from antiquity. The second section focuses on demographic models and life tables. Parkin skillfully explains how life tables are constructed, how they can be employed, and how they can assist the ancient historian in reconstructing possible past demographic systems. In the third section, Parkin presents some "demographic impressions of the Roman world." He discusses issues ranging from mortality and fertility to infanticide and infant exposure to breast-feeding and contraceptive techniques. On every topic, Parkin renders balanced and informed judgments.

This is a good book. It will have a wide audience among ancient historians, and the Johns Hopkins University Press should be encouraged to bring out a paperback version as soon as possible. The book is ideal for use in graduate seminars. Parkin is to be congratulated. His book makes a genuine contribution to the development of ancient history.

THOMAS W. GALLANT
University of Florida

P. J. HEATHER. *Goths and Romans, 332–489.* (Oxford Historical Monographs.) New York: Clarendon Press of Oxford University Press. 1991. Pp. xi, 378. $81.00.

Books about Goths have poured out for the past decade or more. This one, by P. J. Heather, mainly about Goths in the Balkans, recommends itself by dispensing with the influential but anachronistic testimony of Jordanes (author of a synthetic Gothic history, ca. 550) and believing contemporary evidence instead. Among other things, Jordanes gave a distant past to the Visigoths and their dynasty, as well as to the Ostrogoths and their Amal kings. Heather dismisses these claims to antiquity for entities that, he maintains, coalesced only inside the boundaries of the empire. Other Jordanean interpretations are ousted. This long overdue decontamination marks a moment in studies of the Goths and forces changes in currently standard narratives.

Providing an account of this period, especially the fifth-century segment, is like assembling a jigsaw puzzle that lacks at least two-thirds of its pieces. Heather is skilled at joining the pieces that fit. Complicated, poorly documented sequences, such as the triangle involving Theodoric Strabo, Theodoric the Amal, and the Emperor Zeno, are ably reconstructed. Heather is even better at making us respect short-term motives—decision makers preoccupied with the exigencies of tomorrow, or with retaining their thrones, rather than with majestic abstractions, such as the welfare of the empire.

This good training and common sense fail Heather when implementing a complementary agenda. The Goths, it seems, have to be tribal, whatever that means; Heather's notions remind me of how Europeans in Africa applied a fanciful book-learned concept of tribes. He thinks that gift-giving and banquets were peculiarly Germanic practices (that the Goths were not "Germanic" to themselves or the Romans goes unsaid). Romans, he believes, were firm foes of tribal life, and Goths stubbornly defended their independence. Others will ponder how this Gothic obstinacy is compatible with their instant conversion to the Christianity of the empire. Heather is determined that Goths, eager to grow crops, sought fertile land from the empire in preference to other forms of support; even a distribution of freehold lands to individual Goths in 382 is thought possible. How it happened that, when settled somewhere, they left almost immediately and took again to the roads is another loose end. Demography is also on Heather's agenda. Counting people is admirable but pointless unless the method by which the totals of Goths are attained is explained and made plausible. No explanation is given here.

Heather's book clings to the Balkans; Alaric's Goths, for example, after leaving finally for Italy, are allotted only a thin summary. Critical sense is sometimes slighted: a group of panegyrists, whose limitations Heather knows, suddenly turn into reliable spokesmen for how ordinary Romans felt about Goths and other foreigners; Orosius, writing in 417, allegedly reflects a typical belief of the 390s (as though Alaric had not taken Rome in between); and a statement by the Emperor Honorius in 409 (a year after the downfall of Stilicho) is supposed to document his state of mind in 402. Jordanes does not have a corner on anachronism. The seventy or so pages of methodological polemic, often disingenuous, with which Heather begins can be safely by-passed by readers interested in barbarian invasions rather than learned dissension. Heather's study is both a major

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work whose findings merit serious attention and a conspicuously uneven first book.

WALTER GOFFART
University of Toronto

COLUMBA STEWART. *"Working the Earth of the Heart": The Messalian Controversy in History, Texts, and Language to AD 431*. (Oxford Theological Monographs.) New York: Clarendon Press of Oxford University Press. 1991. Pp. xi, 340. $79.00.

This study in theology and intellectual history is meant for scholars and advanced students of early Christian spirituality. Despite his book's subtitle, Columba Stewart offers much more than merely an inquiry into the controversy surrounding the Messalians, the "praying people" condemned as heterodox by the ecumenical council of Ephesus in 431. Indeed, Stewart provides a painstaking analysis of the spiritual vocabulary of late-antique Christianity in the eastern Mediterranean, especially the terminology connoting a believer's confidence or assurance (*plerophoria*) in the feeling or sensation (*aisthesis*) or experience (*peira*) of personal union with Christ or the Holy Spirit. Much attention is also given to the late fourth-century author of Greek spiritual texts known as Pseudo-Macarius, whom some controversialists have subsequently branded as a Messalian.

In a short initial chapter the author outlines some of the past methodological pitfalls that have hindered a proper appraisal of the Messalians. Any distinctive religious ascetical practice expressed in untraditional terminology risks being characterized as deviant by institutional religious authorities. Scholars have too frequently accepted contemporary characterizations at face value, refusing, as the victorious orthodox disputants had done, to accept these practices within their lived context. Stewart wishes to pursue the suggestion that the Messalians' official condemnation resulted largely from the inability of Greek-speaking ecclesiastics to appreciate the Syriac ascetic milieu that gave birth both to the movement and to the writings of Pseudo-Macarius.

In his second chapter Stewart assembles the written testimonies of those fourth and fifth-century sources that mentioned or described the Messalians. Of special significance here are the various lists of doctrines or practices ascribed to the Messalians by their opponents: the presence of an indwelling demon in the human soul that can only be expelled through prayer; the subsequent communing of the Holy Spirit or Bridegroom with the cleansed soul; the liberation of that soul from passions (*apatheia*); avoidance of work; excessive desire for sleep; and a general disregard for the institutional church.

Pseudo-Macarius's own spiritual vocabulary provides the focus for the chapter that follows, especially the Greek terms mentioned above. Although much of this terminology can be found in other Greek authors, the frequency and distinctive utilization of certain arresting metaphors and phrases by Pseudo-Macarius stand out. At this point Stewart declares his intention to explore Greek and Syriac texts in the second half of the book (chaps. 4 and 5) to elucidate a Semitic background to both Pseudo-Macarius's and the Messalians' language of spiritual experience.

Chapter 4 exhaustively analyzes the Greek vocabulary of Christian experience in the Septuagint and Greek New Testament, the writings of the Greek Fathers, Origen, Pseudo-Macarius, and others. Stewart concludes that while these Greek terms themselves remain Pseudo-Macarius's own distinctive trademarks, the "experiential thrust of the Ps.-Macarian vocabulary is thoroughly characteristic of Syriac literature" (p. 168).

The necessary inquiry into Greek and Syriac imagery of spiritual experience (mixing or blending, indwelling of sin and of the Spirit, filling with sin or the Spirit) occupies the fifth chapter. Here Stewart surveys not only the works investigated in the prior chapter but also early Syriac religious literature: the Peshitta version of the Bible, the anonymous *Odes of Solomon* and *Acts of Judas Thomas*, the works of Ephrem, and especially the anonymous *Liber graduum*. Stewart concludes that Pseudo-Macarius did not so much directly borrow language and imagery from Syriac sources as instead share with those sources a theological and linguistic background of common themes, a graphic, almost "material" texture of expression, and a like emphasis on the vivid experience of sin and of the Spirit. Pseudo-Macarius communicated these themes, expressions, and experiences with a distinctive terminology that he "discovered" within the Greek language.

In the sixth and concluding chapter Stewart draws the disparate threads of his investigations together. The shadowy Messalians represented "an irruption into the Hellenistic world of ascetical practices and imagistic language far more characteristic of Syriac Christianity" (p. 234). Scholarly attempts to link Pseudo-Macarius to the actual Messalian movement are both futile and unnecessary. More important is the fact that the Messalians, Pseudo-Macarius, and others all participated in a poetic, symbolic early Syriac Christianity unfamiliar to and misunderstood by sophisticated Greek-speaking theologians and bishops. Thus, Stewart asserts that his book tells but one part of a larger and still relevant story, namely that of "the transmission of spiritual perspectives across linguistic borders" (p. 240). Eleven appendixes and four indexes document the author's philological inquiries.

ROBERT C. FIGUEIRA
Lander University

MEDIEVAL

MARY J. CARRUTHERS. *The Book of Memory: A Study of Memory in Medieval Culture*. (Cambridge Studies in Medieval Culture, number 10.) New York: Cam-

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bridge University Press. 1990. Pp. xiv, 393. Cloth $59.95, paper $22.95.

This book by Mary J. Carruthers is a tribute in both style and content to its two dedicatees, Morton Bloomfield and Talbot Donaldson. It is learned and dense in the Bloomfield tradition, and filled with fascinating bits of information that would have enchanted Donaldson. It teaches more than most would dream could be known about memory in the Middle Ages, but even more important it suggests much about how medieval scholars approached their studies and their compositions. Carruthers contrasts the modern tendency to think of the imagination as the highest creative power with the respect ancient and medieval people reserved for the memory. Memory was valued in the Middle Ages because writers and thinkers relied heavily on earlier works, not in slavish imitation but as starting points or supports for their own thoughts; they used memory not to store words or facts by rote without comprehension, but to absorb material, to analyze it and divide it in ways that made it easy to retain, so that it became part of their thinking and could be retrieved and used where pertinent, in an ongoing dialogue, a continuous increasing and revision of thought. The use of earlier texts does not preclude originality; they are simply part of the material the mind uses to form new thoughts.

The proof of a good memory in the Middle Ages is not the ability to regurgitate masses of material but the ability to incorporate and grow from what has been consumed and digested, and food metaphors are frequently used for the process. There is a distinction between memory of words and memory of "things" (*memoria ad res*), getting the gist of the material, which is far more valuable than remembering specific words. A key difference between medieval and modern habits of scholarship is the approach to others' thoughts and works as common instead of private property. Whereas we worry about plagiarism, about the appearance of stealing ideas and words, they felt free to draw on any relevant source, not needing to acknowledge it because they could assume others would recognize it from their own memory stores, and furthermore they expected their own writings to be taken up and used by others in the same way. Authors became authorities when their works were retained in the memory of subsequent generations and imitated.

It is significant that Carruthers throughout the book is concerned with literate and not oral traditions, with people who worked with written texts, but who drew on those texts by sifting through their memories, not by leafing through manuscripts. It was a sign of command of the material to be able to evoke what was needed from the memory, either when speaking—particularly if one were a preacher or a lawyer—or when composing a written text. Hence, and this is an important finding for modern scholars of medieval texts, a paraphrase of a text rather than a direct and accurate citation does not necessarily indicate that the author did not know the text or was working from a corrupt source. More likely it means that this was the meaning the author chose to give it or the form that best suited the particular context.

There is unfortunately no space in a short review to do justice to the etymological gems that turn up from time to time: that "quote" comes from *quot*, "how many," because texts were subdivided by numbers to aid the memory; that the mnemonic wheel of Virgil (*rota Virgili*) may be the source of learning "by rote." Nor can one do justice to intriguing suggestions about well-known literary passages, such as, for example, when Chaucer's monk speaks of the tales he has in his "celle," he is referring to the storeroom of his memory, which he can call on during the journey, not to the room he lives in.

It is virtually impossible in a book so filled with details and so wide-ranging in its scope not to find points of differing magnitude to quibble with: Hildegard is a twelfth, not eleventh-century figure (p. 290); to say that Heloise is a "subject-who-remembers," a self "constructed out of bits and pieces of great authors" (p. 180), because she uses Cornelia's speech from Lucan at a crucial moment in her life, is to deny the strong personality which chose that particular and in many ways shocking passage from a pagan author to make a public statement as she took the veil. But it is also impossible not to learn a good deal from this book, which discusses basic models for memorizing knowledge, the relation between sensory perception and intellectual abstraction, schemes to train the memory, the role of memory in reading, composition, and the layout of texts in physical books; which offers translations of three passages on memory in appendixes (from Hugh of St. Victor, Albertus Magnus, and Thomas Bradwardine) and thirty black-and-white illustrations, most of them clear and wonderful.

JOAN M. FERRANTE
Columbia University

PAOLO PRODI. *Il sacramento del potere: Il giuramento politico nella storia costituzionale dell'Occidente.* (Annali dell'Instituto storico italo-germanico, number 15.) Bologna: Mulino. 1992. Pp. 602. L. 60,000.

The history of oaths in the West is a vast subject, as the extensive bibliography included in Paolo Prodi's erudite book makes evident. Often confused with a vow, which is a voluntary promise to God, an oath calls on God to testify as a witness. Oath-taking in the medieval West was especially pervasive. After reading the *Song of Roland*, or Galbert of Bruges's chronicle on the life and murder of Charles the Good of Flanders, one is easily persuaded that medieval men and women were oathaholics, always ready and willing to invoke divine testimony to validate their claims. Oaths provided divine justification for judicial pro-

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ceedings, officeholding, commercial relations, and formal contracts, as well as for informal arrangements. They could also be coerced. As Bernard Gui's famous manual for inquisitors indicated, individuals tried for heresy were compelled to take oaths, so that God's will decided whether the accused were justified or guilty; otherwise it would be impossible to tell if they had committed perjury. Subject to murderous harassment in the later Middle Ages, Jews were forced to take a special, humiliating oath declaring their innocence when they appeared in Christian courts as defendants or plaintiffs. This practice lasted into the nineteenth century. A theoretical foundation for oaths was provided by theologians such as Thomas Aquinas, who warned that the things to be confirmed by oath must be true and not sinful and that there was danger in taking oaths for frivolous reasons.

Clearly, the documentary record of oath-taking is open-ended and particularly rich for anthropological history, but that is not what Prodi has in mind. As a distinguished historian of medieval and early modern ecclesiastical and political theology and institutions, Prodi has chosen to focus on the oath as the transcendent justification of the vertical ties binding ruler and subjects and the horizontal ties binding citizens within a city or state to each other. Prodi classifies his work as wide-ranging constitutional history, taking his inspiration from recent German historiography or *Verfassungsgeschichte*. His conception of the oath as an expression of political theology is also sparked by the recent vogue among constitutional theorists in Italy for the political and legal theories of Carl Schmitt, a self-styled antiparliamentary democrat in Weimar Germany who later embraced the Nazis. The oath played a central role in Schmitt's vision of constitutional order, and his formulation that "all significant concepts of the modern theory of state are secularized theological concepts" was taken as gospel by his disciples and non-Marxist antiliberals.

Prodi's central thesis is that the oath (*sacramentum iuris*) represented not a frozen, metahistorical reality, as is often thought, but a dynamic reality whose history is a concrete manifestation of the interaction of theology, politics, law, and institutions. Early chapters show how, with the help of Jerome and Augustine, the oath was transformed from a pagan into a Christian rite, how it became subject to penitential discipline, and how perjury became equated with the crimes of theft and homicide. The author's characterization of medieval Western Europe as a "*società giurate*" is persuasive, as is his argument that ecclesiastical division of the world into sacred and temporal or law spheres was ultimately responsible for the secularization of political oaths. Later chapters deal with the increasing secularization of the political oath in the early modern period, leading to Benjamin Franklin's quip that the oath is the last recourse of liars. Oaths, Prodi concludes, continue to play a ritual role in politics and courtrooms everywhere in the West, but they have lost their hold on our imaginations. The decline of the political oath is ironically exemplified by Italy's contemporary *Lega Lombarda*, which routinely stages its oathless, secular rallies at Pontida near Milan, the site where the cities comprising the original *Lega Lombarda* met in the twelfth century to take a solemn oath to act in concert against the Holy Roman emperor.

A brief review cannot do justice to this excellent book, from which historians of every period can profit. The absence of an index, however, is a serious drawback. Finally, I missed a discussion of the eighteenth chapter of Machiavelli's *Prince*, on when rulers should keep and break their promises, a foundational text of early modern political thought.

JULIUS KIRSHNER
University of Chicago

JOHN MARSHALL CARTER. *Medieval Games: Sports and Recreation in Feudal Society*. (Contributions to the Study of World History, number 30.) New York: Greenwood. 1992. Pp. xiii, 159. $42.95.

John Marshall Carter has written more on medieval sports and pastimes than anyone, and generally these works have presented solid and original historical research. It seems, however, that Carter's search for a history of medieval leisure activities is a lonely one. He begins his latest work, a collection of short essays, with a historiographical essay, "The Study of Medieval Sports and Recreations, 1927–1991," which establishes that, despite the efforts of the great medievalist Charles Homer Haskins in the early part of this century, few medievalists have chosen to study medieval sports and pastimes. Carter seems to have the field to himself; thus, the historian reading this book might anticipate a feast of almost virginal knowledge about medieval fun. Unfortunately, this collection is not as historically fulfilling as his previous works.

It is true that two essays ("Sports, Recreations, and the Church in Feudal Society" and "Sports Violence in Medieval Society") are good examples of the distinctive historical research expected from Carter. The first introduces the reader to the medieval church's almost continual dislike of chivalric recreational activities, especially hunting, hawking, and tournaments, although there were always some clerics who preached acceptance of knightly sports, and even others who openly participated in them. The second essay traces through legal records the incidence of violence that sometimes accompanied sports and pastimes in thirteenth-century England—almost all connected with the large quantities of alcohol that were consumed during these events—citing not only the sports during which violence broke out but also the social status of the participants involved in these outbreaks.

The remainder of the essays in this collection,

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however, do not match this standard of excellence. Most are simply short "thought" pieces, lacking much substance, while two, "Sports, Recreations, and War in Feudal Europe" and "Sports and Recreations Reflected in Art," are virtual repetitions of previously published material.

But perhaps no essay in this work is so poorly conceived and presented, and so filled with speculative titillation, as "Sports and the Public Person: Establishing a Reputation and Promoting It in the High Middle Ages." This entire essay seems founded on the idea that "if a lady chose a particular knight as her champion, she had to reward him with her body if he won" (p. 41), a declaration for which even Carter has to admit he has no evidence. Yet not only does he continue to build on this ill-informed speculation throughout the essay but he also concludes it with a fabricated dialogue between a veteran of hundreds of tournaments and a rookie, which ends: "Young man, I tourneyed with William Marshall in France for a number of years; we broke hundreds of spears together; we drank and womanized together all over Europe. And, young man, you're no William Marshall" (p. 46).

In this book John Marshall Carter is no Charles Homer Haskins; indeed, he is not even the old John Marshall Carter. Hopefully, in future work he will try to imitate his past work and not his present.

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GEOFFREY KOZIOL. *Begging Pardon and Favor: Ritual and Political Order in Early Medieval France.* Ithaca: Cornell University Press. 1992. Pp. xxiii, 459. $44.95.

In this highly original book, Geoffrey Koziol studies the language and gestures of petition and supplication in tenth and eleventh-century northern France. His central argument is that political order, both in the secular and the ecclesiastical spheres, was closely dependent on ritual: kings governed, relations between antagonists were resolved, and monks had their property rights confirmed through grand gestures of humble supplication. Because power, including secular power, was considered to derive from God, it was appropriate to petition the mighty in attitudes of prayer; yet even the mightiest were Christians who would go on their faces before God's agents. Koziol distinguishes carefully between choreographed "ceremony," which he finds uninteresting, and a highly flexible system of "ritual," related to but not limited by the liturgy of penitence. One of his principal themes is that the late Carolingian and early Capetian period, which has often been characterized as violent and even anarchical, was in fact held together by such ritual, providing an explanation for why the society did not simply collapse.

Koziol bases his analysis on a wide variety of sources, including charters, visual images (the book includes a nice selection of illustrations from medieval manuscripts), and contemporary annals. He finds the chronicles of Richer and Flodoard, the history of the Norman dukes by Dudo, and the biography of Robert II by Helgaud especially useful. His bibliography includes an impressive array of secondary works and primary sources, including both edited and manuscript sources. No one has studied this particular topic before, and yet when reading this book medievalists will realize that they should have been seeing these rituals all along. It was part of the tension within medieval thought that the greatest effect was sometimes achieved by the union of opposites. New men, including the knights of the eleventh and twelfth centuries, were supplicated, as were long-established lords, yet at the same time men newly risen to power could demonstrate that power best by throwing themselves humbly at the feet of those above them, or even their social equals. And if one was backed into a corner, or fighting for one's political life, often the best offense was total surrender.

Although the overall ideas are fascinating and indeed convincing, the book itself remains somewhat awkward. Koziol tells us in his preface that the topic evolved a long way in the years that he was writing the book, and its extended gestation may explain some of these problems. For example, he says that he originally intended to write on conflicts between monks and nobles, and the chapter on this topic contains bald (and perhaps residual) statements such as that powerful lords were always monks' antagonists, violators of monastic immunities, and the monks never could understand them. These oversimplifications, however, are contradicted by his more careful treatment elsewhere of lay-ecclesiastical relations.

One would assume that Koziol chose northern France because this area showed his rituals most clearly, yet he seems uncomfortable with his choice of region. His "northern France," according to the map at the beginning, is a broad swath stretching from Poitou and Anjou to the Ile-de-France and Flanders, but it also includes a disjunct piece of southern Burgundy. At different times (and for reasons not always clear) he selects quite different areas of "northern France" on which to focus, most frequently the archdiocese of Reims, but sometimes the region from Paris and Reims north into Flanders, and sometimes the Blois-Chartres region, which has the entire Paris and Champagne regions between it and the archdiocese of Reims. It seems curious that for his discussion of eleventh-century property disputes he chose to analyze documents from Picardy only, rather than from elsewhere in northern France, given what he apologetically calls the "poverty" of sources in the Picardy region (p. 325). In contrast, Stephen D. White, studying disputes in the same period, found substantially more sources by concentrating on Anjou instead (*Custom, Kinship, and Gifts to Saints* [1988]). In spite of the broad area whose documents he studied,

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Koziol also and for unexplained reasons plucks a number of illustrative examples from elsewhere, for example from Germany, southern France (the Auvergne and Périgord regions), and even the Scandinavian sagas and late-medieval commercial London (for example, pp. 29, 39, 59, 66, 101, 311).

In a book that focuses on the early Capetians, it is unfortunate to see some slips in Koziol's treatment of them, such as making King Raoul the brother-in-law rather than son-in-law of Robert I (p. 112), or finding Hugh Capet's desire to affiance his heir to a Byzantine princess "ludicrous" (p. 125), when Hugh's own letters make clear his reasons (there was no woman in the West of sufficient status who was not also his cousin). One also regrets that, although he says that rituals of supplication began in the Carolingian period and changed substantially in the 1120s, he discusses neither the origins nor the evolution of these rituals. But in spite of such reservations, this book remains a stimulating and thought-provoking contribution.

CONSTANCE B. BOUCHARD
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VERONICA ORTENBERG. *The English Church and the Continent in the Tenth and Eleventh Centuries: Cultural, Spiritual, and Artistic Exchanges*. New York: Clarendon Press of Oxford University Press. 1992. Pp. xv, 328. $79.00.

Veronica Ortenberg's book is about the exchanges of individual English churchmen and their communities with clerks and monks at important ecclesiastical centers across Western Europe and the Christian Mediterranean. As such, it is a broad-ranging and ambitious study, full of information on ecclesiastical links between England and its neighbors. In order to accommodate such a large subject to a single volume, Ortenberg has necessarily narrowed her investigation in a number of ways. She has not included England's links with Scandinavia and the Baltic, nor does she spend much time on the Anglo-Saxons' relations with their Celtic neighbors, some of whom were settled on the Continent. Nor does her book study the relationship between England's secular aristocrats with Continental cults and monastic communities. The work is also not about the shared mental world and overlapping textual communities found in England and abroad.

What Ortenberg does is trace the shared cults, liturgies, iconographic language, and monastic customs of English churchmen and their Continental friends, primarily in the tenth and eleventh centuries, although there is much information on earlier years as well. The book is divided geographically into chapters on Flanders, the empire, Italy (except Rome), Rome, Byzantium, and France. Each chapter is then further divided into sections offering a brief geographic outline, narrative evidence for contact, cultural exchanges, liturgical and devotional borrowings, and artistic relations. This structure has a tendency toward repetition and gets in the way of broad analysis, but its advantage is that it makes this book easy to negotiate (no mean feat with such complicated and dense material) and an important work of reference in which to find leads and locate specific kinds of borrowings.

As Ortenberg travels through Europe, she describes in detail the churches, wall paintings, shrines, and liturgical spectacles that would have greeted English travelers on their way to Rome, Cluny, or Trier; she also reconstructs the kinds of cults and lavish church art with which ecclesiastical impresarios who traveled to England for a year's sojourn or a decade-long career would have encountered. Her detailing of the transmission of cults and liturgy is impressive and provides a strong core to the book. Her sections on Rome, the rest of Italy, and Flanders are most interesting, thorough, and thought-provoking. Less successful is her section on France, where, among other things, the author only partially describes England's pre and post-Conquest connections with Normandy and says little about the kingdom's interesting and persistent ties with Aquitaine. Nonetheless, Ortenberg's book is interesting, useful, and beautifully written, and it shows that the English church in the tenth and eleventh centuries, far from being the isolated institution portrayed by Anglo-Norman monk-historians, stood firmly in the mainstream of Christendom.

ROBIN FLEMING
Boston College

CHARLES M. RADDING and WILLIAM W. CLARK. *Medieval Architecture, Medieval Learning: Builders and Masters in the Age of Romanesque and Gothic*. New Haven: Yale University Press. 1992. Pp. xiii, 166. $37.50.

Forty years ago, in his *Gothic Architecture and Scholasticism* (1951), Erwin Panofsky attempted a comparison of the products of medieval masters and builders. Although Charles M. Radding and William W. Clark's point of departure, like others since 1951, is Panofsky's underlying idea, they have chosen a different focus of analysis. Rather than concentrate on literary and architectural products, Radding and Clark argue that we must focus instead on the masters and builders themselves, and not biographically but conceptually. As a means of doing so, they identify two bases for comparison: the disciplinary bonds that united masters and builders in well-defined communities, and the cognitive habits (to borrow a medieval term) that distinguished the Romanesque and Gothic periods from the early Middle Ages.

Radding and Clark investigate the disciplinary unities of these two groups first from the professional behaviors of their members: the degree to and speed with which individuals used or responded to the

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innovations of their predecessors and contemporaries. They argue that doing so allows us to distinguish three essential stages of development in the eleventh and twelfth centuries. In the first, from the beginning of the eleventh into the early twelfth century, the principal achievement of builders was the perception of space as multiples of smaller units, so that design reflected the requirements of volumes. In the same way, the authors note (as have others before them) that eleventh-century scholarship reflects a cognitive transformation from assertoric to argumentative styles. In both cases, the crucial element is not the creation of solutions but a reformulation of problems.

In the second stage, characterized by the work of Peter Abelard and the Abbot Suger, masters and builders put aside works devoted to single issues or problems and began to build systematic treatises or comprehensive designs. In contrast to the earlier period, when builders or masters worked sequentially—in architecture, designing the floor plan first, then the walls, windows, and vaults; in scholarship, glossing or analyzing individual texts—the builders and masters of the 1130s began to conceive of design and scholarship comprehensively, so that vault design must be considered in the floor plan or psychological theory in soteriology.

The final stage, in which fully established disciplines emerged, occupies the second half of the twelfth and the first two decades of the thirteenth centuries. The significance is to be found less in individual genius than in collective consensus: that thirteenth-century theologians and builders might construct *summae* or cathedrals so familiar to us is due in large measure to the experimentation undertaken in the late twelfth century and the ultimate consensus that was achieved then. Although Radding and Clark's treatment of this experimentation might have been expanded to include axiomatic theology like that seen in the late twelfth-century *Regulae Theologiae*, or didactic techniques in the Euclidean tradition, the examples provided make the point quite well.

One might, of course, quibble about the periodization of this interpretation. One suspects that the moment at which these cognitive plateaus were reached varied with the particular discipline, reflecting in part the works translated and thus the available knowledge of the ancients. It is also clear that the work has drawn on Thomas Kuhn's *Structure of Scientific Revolutions* (1962). The authors refer to Kuhn's work in their introduction, and elsewhere employ the notions of paradigm and disciplinary matrix, both central features of his work. Such references may be incidental, an additional example of the degree to which Kuhn's ideas have spread beyond the history and philosophy of science. But one might also be concerned that in invoking Kuhn's program, the authors have tied their work to other positions in his book that have little or no application to the matter at hand.

Such reservations should not detract from this otherwise fine study of the intellectual and artistic emergence of Western Europe. Its thought-provoking thesis and the clarity and breadth of its presentation will provide other scholars with a foundation and template for similar work.

Steven J. Livesey
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Anne L. Clark. *Elisabeth of Schönau: A Twelfth-Century Visionary*. (Middle Ages Series.) Philadelphia: University of Pennsylvania Press. 1992. Pp. x, 211. $28.95.

This biographical study of the twelfth-century mystic and Benedictine nun, Elisabeth of Schönau (d. 1164–65), offers an informative introduction to one of the lesser-studied German women mystics. Anne L. Clark succeeds in bringing Elisabeth out of the shadow of her famous contemporary, Hildegard of Bingen, by arguing against contemporary scholarship that Elisabeth was not simply an imitator of Hildegard but instead a bold and original visionary in her own right. Clark also succeeds in extricating Elisabeth from the conventional view of her as the "mouthpiece" of her scribe and brother, Ekbert of Schönau, by producing a careful reading of their interaction in the written texts that survive. This argument is one of the most exciting and fruitful in the book.

Clark's aim is to "present a comprehensive examination of the extant records of Elisabeth's experiences" (p. 2). Beginning with a survey of the mystic's life, Clark then describes the visionary texts she dictated to Ekbert, including three visionary diaries, a collection of moral sermons inspired by Hildegard of Bingen's *Scivias*, a popular book of visions about the martyrdom of Saint Ursula and the 11,000 virgins, and a theologically problematic vision of the corporeal assumption of Mary. Clark divides her discussion of Elisabeth's mysticism in chapters 5 and 6 into her "visionary experience" and her "religious vision," categories that I find confusing. Nevertheless, these chapters include apt discussions of the Christian tradition of vision and prophecy, mystical language and symbols, categories of mystical experience, and Elisabeth's evolving visionary scope. The chapter on Elisabeth's "religious vision" focuses on her experience of the "other world," that is, of Mary, the saints, and the heavenly kingdom.

Although Clark's analysis benefits from contemporary scholarship in religious history and medieval mysticism, she avoids engagement with much recent work on specifically female piety, including Carolyn Walker Bynum's, with which she claims to be in agreement. Although Clark aims to contextualize the life and writing of Elisabeth of Schönau, she does not look to contemporary female piety for that context. In fact, she often distinguishes Elisabeth's mysticism from the evidence of female mysticism in the twelfth century. Clark's contextualization relies more on biblical, hagiographical, and theological traditions of

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prophecy and vision than it does on contemporary female experience. I found this exclusive emphasis (except for her discussion of Hildegard) both puzzling and misplaced. Where issues related specifically to women's mystical experience, such as Christ's femininity, come up in the visions of Elisabeth, Clark offers little comment about their relevance to women's piety in the twelfth century. Yet at the end of her book, Clark makes the astonishing claim that Elisabeth's visions have more in common with women's visionary literature of the thirteenth and fourteenth centuries than the tradition she has already mapped out. I would agree, but I think more explicit comparisons with that later tradition need to be made in this book.

Finally, the relationship of Clark's book to feminist scholarship is troubled. She acknowledges that her work "builds" on the efforts of feminist historians to expose the "patriarchal structures" of the medieval church, yet she sees her own biographical project as lying within the realm of the "less anachronistic theoretical framework" of Bynum's work. Whoever the "straw-feminist" is here, Clark needs to articulate and develop the "less anachronistic" theoretical argument that she claims to be offering.

KARMA LOCHRIE
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PAUL BRAND. *The Origins of the English Legal Profession.* Cambridge, Mass.: Blackwell. 1992. Pp. ix, 236. $74.95.

The origins and early development of the English legal profession have long waited for a full scholarly treatment, and Paul Brand has provided one. He sees Anglo-Norman England as "a land without lawyers," meaning that they were not needed because of the simplicity of the legal process, or that courts did not permit their use.

With the legislation of Henry II, new royal courts, systematized in their operations, provided conditions for the use of professional litigation agents. Operating from the late twelfth century, these royal courts offered continuous sessions that permitted a greater volume of business, kept written records, and required that litigation be initiated by a royal writ emanating from the king's chancery.

To the exchequer, to the eyre courts, and to the common bench of the late twelfth century were added, in the thirteenth century, the king's bench and the assize courts, the former moving with the king and the latter pursuing their business in periodic circuits. But the general eyre circuits became less frequent in the thirteenth century, and this change was a major influence in bringing an ever-increasing volume of litigation to the common bench which, in breaking away from the exchequer, conducted business in four annual judicial terms rather than in the two financial terms of the exchequer.

Other changes occurred in the litigation process. The number of royal writs available for initiating lawsuits increased from about fifteen in 1200 to sixty-five in 1270 and stood at more than one hundred in 1300. This increase made it difficult for the litigant to choose without assistance the correct writ for his particular case. Equally important in the pleading process was the growth of exceptions. In the early thirteenth century, the plaintiff stated the plaint or "count" adhering strictly to the specifics of the writ. The defendant ordinarily denied these specifics. In the course of the century, defendants or their pleaders challenged more often the accuracy of the specifics and even the validity of the writ. An entire body of exceptions came to be allowed by the court, and the plea became a series of arguments that demanded the use of legal experts.

The attorney who usually obtained the necessary writs and "stood for" his client during the legal process was in clear view at the beginning of the century and was often a friend or a relative of the litigant. By the end of the century, he was a professional, serving a large number of clients. The more important legal agent was the *narrator* or the serjeant who gave the count in court for his client, and who pleaded and argued the case. Not in view at the beginning of the century, he matured as a professional during the reign of Edward I. Although we see hundreds of attorneys in the plea rolls of the common bench by 1300, the serjeants became and remained a small group, numbering about thirty practitioners in any judicial term.

The thirteenth century saw not only the development of the professional English lawyer but also witnessed the bifurcation of the English legal profession, a situation that has lasted to this day. Attorneys, as ancestors of the modern solicitor, have remained numerous. The serjeants, as ancestors of the modern barrister, have remained a small and highly specialized group.

FRANKLIN J. PEGUES
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STEVEN A. EPSTEIN. *Wage Labor and Guilds in Medieval Europe.* Chapel Hill: University of North Carolina Press. 1991. Pp. vii, 307.

Medieval guilds have long caught the attention of students of the Middle Ages. Although there were early seminal studies of guilds by scholars such as Emile Coornaert, Sylvia Thrupp, and André Gouron, it is only in the last two decades that case studies of the guild structures at specific European sites have begun to appear in great number. In this book and in an earlier article, Steven A. Epstein joins the growing ranks of scholars focusing on some aspect of the organization of work in the Middle Ages, on the one hand, and revisiting the topic of guilds, on the other. Epstein takes all of Europe as his arena, and the

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breadth of his reading in primary and secondary materials for the British Isles, France, the Low Countries, Spain, and Italy gives justification to his general title. His emphasis is overwhelmingly urban.

Epstein investigates the historical background for guilds, the case for continuity from the Roman period, and the emergence of guilds in the post-Carolingian period. Epstein is aware of the shortcomings of surviving guild statutes for a study of origins. He scrutinizes the internal workings of the guilds and the place of guilds within the broader political and social context. He examines the experience of guilds and wage labor during the era of crisis and change in the later Middle Ages. His interesting points of discussion in this later period include reflections on the fruits of wage labor, particularly in the adoption of technological innovations, the effect of the mobility of wage labor on the spread of invention, the pre-plague stagnation of wage levels, the role of wage labor and guilds in urban revolts, and the triumph of wage labor over slavery in the economy of medieval Europe. This is a more broadly based work than the study of Richard Mackenney on guilds in Venice and Europe (*Tradesmen and Traders: The World of the Guilds in Venice and Europe, c. 1250–c. 1650* [1987]). Yet by no means does Epstein have an exhaustive command of recent literature on the guild system. For example, there are significant lacunae in the French historiography of the topic. Epstein's conclusions must be considered tentative and subject to revision in individual case studies. His effort to generalize remains, nonetheless, a worthy one.

Epstein comes to the topic of labor and guilds from a deep acquaintance with the notarial records of medieval Genoa. His earlier article explored the issue of work and wages in the port setting of that medieval commercial giant ("Labour in Thirteenth-Century Genoa," *Mediterranean Historical Review* 3 [1988], 114–40). Genoese experience is brought to bear at each juncture of this study. Where Epstein seems most insightful is in issues which pertain to that Ligurian capital, as in his treatment of the impact of the rhythms of the Mediterranean sailing season on patterns of employment.

To his credit he has construed the topic of labor and guilds in an abstract as well as empirical fashion and spends considerable effort investigating the writings of medieval thinkers to ferret out any surviving opinions. He is inspired in this regard by the superb and subtle distillation of Anthony Black (*Guilds and Civil Society in European Thought from the Twelfth Century to the Present* [1984]). Epstein finds relatively little grist for his mill since few medieval thinkers devoted significant space to the issues of guilds or work. Nonetheless, Epstein identifies an increasing valuation of work from the thirteenth century, thanks to scholars such as Bonaventure, who elevated the concept of work by viewing God as "an archetypical worker." He creatively explores such sources as examples for further insights on labor.

Regrettably, Epstein's study is plagued by several problems. Inconsistencies appear in regard to some issues such as day-rate and piece-rate wages. Stylistic problems and imprecision of thought mar the book, such as this sentence: "Freeing *employer* from its modern connotations and seeing the word as ambiguous and open-minded about the different ways to profit from labor is necessary" (p. 49). The discussion of the status of different guilds is particularly diffuse and confused. Peculiar spellings such as *chief d'oevre* occur occasionally, and, on pages 136–37, one finds four different spellings of the term *prudhommes*. There also are some outright historical errors, such as Epstein's inclusion of Marseilles as a part of Capetian France in the thirteenth century (p. 196). These weaknesses make the book less profitable than it might have been. Still, Epstein has made an admirable and ambitious effort at synthesis in the area of medieval guilds and urban labor.

KATHRYN L. REYERSON
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R. H. HILTON. *English and French Towns in Feudal Society: A Comparative Study.* (Past and Present Publications.) New York: Cambridge University Press. 1992. Pp. xi, 174. $44.95.

This book by R. H. Hilton, one of the foremost students of medieval economic and social history, is a refreshingly broad comparative study of how towns fit into the feudal milieu from about the tenth century to the fifteenth century. Hilton is firm in his belief that feudalism remains a useful concept for medieval historians and is not a tyrannous construct. He goes beyond the lord-vassal relationship to emphasize that feudalism was the social formation in which French and English towns developed from the nadir of urbanization in the early Middle Ages. These towns were not, in Hilton's view, antifeudal entities but instead reflected the values and styles of coercion typical of feudal society. Hilton draws his evidence from northern France and England; he decided for unconvincing reasons to exclude Flanders, and he does not say much about southern France. The evidence comes mainly from the English sources, but Hilton has read widely in the secondary literature on France.

Hilton's argument on how feudalism shaped the rise of towns is subtle and complex. Nobles were active in founding small towns, and, Hilton claims, the role of merchants in urban growth has been exaggerated. Lords owned land in towns and their suburbs, and monarchs and lords continued to have some jurisdiction over towns, especially in France. A chapter on guilds stresses that these institutions were "subordinated organs for municipal control" and not as widespread or effective as scholars have assumed. Hilton's key point is that the oligarchs in the towns

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were not antagonistic to the feudal order. His proof seems to be that the forces that caused the transition from feudalism to capitalism were not located in the towns but rather in the countryside. The urban elites did manipulate taxation to coerce their subordinates in much the same way that lords used rents to expropriate peasants. Merchants dominated English towns, while in France lawyers and royal officials ruled, yet all of these people wanted to be in the feudal hierarchy and not apart from it.

This brief summary does not do justice to Hilton's arguments; given the rich layers of interpretation, there is plenty to ponder in this book. Hilton understands rural society, but his view of class formation in the cities does not have a strong base in recent works on that subject. The law does not enter into Hilton's analysis, and the English common law and Roman legal tradition in France differed on the rights and limits of corporate bodies like communes and guilds. Hilton sees no fundamental differences between the rural and urban economies, just parallel systems of coercion. Yet why did medieval peasants try to run away to cities and what opportunities did the developing wage economy present them? The Capetian policies on communes were more complex than Hilton allows; Paris never had a commune and the crown did not concede much self-rule to its cities. The English experience is more complex, and there is the opportunity for a comparative history that stresses the differences between the two countries. This book challenges its readers to reconsider their own views in the light of the author's analysis.

STEVEN A. EPSTEIN
University of Colorado, Boulder

MODERN EUROPE

VALERIE I. J. FLINT. *The Imaginative Landscape of Christopher Columbus*. Princeton: Princeton University Press. 1992. Pp. xx, 233. $24.95.

The Columbian quincentenary and the anticipated celebrity that Columbus was to have enjoyed in 1992 inspired many writers and presses to bring out books on the Admiral of the Ocean Sea. Although the celebrity turned out to be more negative than they had expected, much useful scholarship also surfaced in 1992. There are few professional experts on Columbus, and others who chose to write about him understandably brought their own interests to bear as they considered aspects of the admiral's career. Valerie I. J. Flint's background as a scholar of medieval ideas is readily apparent in the features of Columbus's life that she emphasizes in her imaginative book.

In the first five chapters, she is concerned with the sources of Columbus's knowledge of the world and the uses to which he put them. To this end she discusses world maps, sea stories, European knowledge of Asia, and speculations about the terrestrial paradise. In the sixth and final chapter, she offers an assessment of Columbus's religious outlook.

Throughout, Flint carefully labels what we know directly from Columbus's own writings about his intellectual formation and also speculates on what we can surmise. This latter effort is fraught with danger. Columbus's life is probably as well documented as any non-noble European of his era, but there are still many lacunae. In the absence of documents that would define him completely, it is all too easy to fall into the pit of asserting what Columbus must have known, read, or believed. The use of such speculation is legitimate when it conforms with what we know of the patterns of his life and the society in which he moved. Lamentably, however, Flint at times carries speculation beyond reasonable bounds. For example, her third chapter, "Sea Stories," is a well-researched retelling of the stories mentioned in books Columbus himself owned or in notations in his own writings. Yet when Flint attempts to show that Columbus must have known the stories of Sinbad the Sailor from the *Thousand and One Nights*, she goes beyond reasonable deduction. No evidence exists that Columbus knew the Sinbad stories, and Flint neglects to show how he could have known them, for she does not tell us how available they were in his time or how frequently they were cited in works to which he had access.

The final chapter on Columbus's religious outlook is perhaps the least satisfactory. Flint carefully attempts to distance herself from the Columbus bashers, but she comes ultimately to a commonplace and erroneous view: that Columbus was insincere, that he manipulated his piety to cover his vast ambitions, and that his piety was designed to hoodwink Queen Isabella. Such a view conveniently ignores two major obstacles. One is that Columbus, throughout his adult life, believed that he received communications directly from God, a fact that Flint neglects. The other is that few people, if any, ever hoodwinked Queen Isabella.

Flint is an erudite scholar of medieval intellectual history, and the best parts of this book discuss the writers we know Columbus read and assess their probable impact on him. Unfortunately, her knowledge of Columbian scholarship is incomplete. She makes various minor errors in Spanish and misunderstands several episodes of Columbus's first voyage. For that voyage she relies on older translations of Bartolomé de Las Casas's summary of Columbus's on-board diary (Cecil Jane's of 1930 and Samuel Eliot Morison's of 1963), rather than the new standard bilingual edition by Oliver Dunn and James E. Kelley, Jr., published in 1989. Her bibliography does not even mention Juan Gil's *Mitos y utopías del Descubrimiento*, whose first volume (*Colón y su tiempo* [1989]) addresses many of the same points as Flint's treatment of the sources for Columbus's geographical ideas. Her account of Columbus's religious outlook

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might have been more persuasive had she considered Delno West and August Kling's new edition of Columbus's *Book of Prophecies* (1991), with its extensive critical apparatus. With more time and better advice she could have remedied these shortcomings, but then the book might have missed the quincentennial window of opportunity.

WILLIAM D. PHILLIPS, JR.
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MASCARENHAS BARRETO. *The Portuguese Columbus: Secret Agent of King John II*. New York: St. Martin's. 1992. Pp. xxii, 572. $35.00.

Our knowledge about Christopher Columbus has expanded as modern scholars have placed him squarely into the regional and global forces at work during the fifteenth century. Generally speaking, scholars agree that Columbus was born in the important and cosmopolitan Republic of Genoa to Christian parents. He became an entrepreneur carrying merchandise between Europe and near ocean islands such as the Madeiras and Canaries, and he ultimately negotiated a business deal with the Spanish monarchs (*Capitulations of Santa Fe*) giving him proprietary rights and profits from all commercial activities in islands and mainlands in the "Ocean Sea" beyond its known boundaries. His birth and early years have been documented from the archives of Genoa and the nearby districts of Liguria, Piacenza, and Monferrato (see, for example, the collections of essays in *Atti del Convegno Internazionale di Studi Colombiani* [1974] and *Atti II Convegno Internazionale di Studi Colombiani* [1977]).

Such scholarship fails to impress nationalistic advocates and Columbus hobbyists who want to claim him for Greece, France, Spain, Majorca, Catalonia, and elsewhere. In this massive book, Mascarenhas Barreto, journalist, broadcaster, and teacher of the sociology of art and the history of discoveries, tries to make the case that Columbus was Portuguese. With pages of genealogical charts, Barreto proposes that Columbus was in reality Salvador Fernandes Zarco, the bastard son of Prince Fernando, grandson of the navigator João Gonçalves Zarco (a converso Jew), grand nephew of Prince Henry the Navigator, and a member of the Colonna family through his mother's lineage. He was born, Barreto claims, in the Portuguese town of Cuba (thus the real reason for the name of the Caribbean island) near Vila Ruiva in 1448. If that were not complicated enough, Barreto further illustrates through more genealogical charts that his so-called brothers, Diago and Bartolomé, were members of the Aguiar family, and that his mistress Beatriz Harana was a third cousin.

According to Barreto, Salvador Fernandes Zarco, who took the code name Christóbal Colón, was a secret agent of King John II. His mission was to distract the Spanish monarchs away from the routes to India being developed by Portugal around the tip of Africa by convincing them to support a voyage across the Atlantic Ocean. Barreto claims that the Portuguese had already traveled to the New World and knew that the way to the Orient was blocked by a huge continental landmass. Columbus's accomplishment was not the ocean voyage of 1492, but getting the Spanish monarchs to sign the Treaty of Tordesillas in 1493.

What about all those documents that survive in Genoa? Barreto reports that they are all false or have been misinterpreted. He also explains that the early biographies of Columbus by his son Fernando and Bishop Bartolomé de Las Casas were massive cover-ups. Barreto points out, for example, that Columbus could not write in Italian (considering that Genoese Italian was not a written language, this does not seem too unusual), and that his education was too good for him to have been the son of an Italian wool merchant. But the real proof of his "true" origins is to be found in the Cabalistic signature he used. "Smoke and mirrors" take on a new meaning when Barreto breaks the code contained in Columbus's triangular signet (p. 286). By holding the signet up to a mirror, an entirely different meaning can be produced from the mysterious letters. ":Xpō ferens./," for example, changes from "Christ bearer" to an encrypted "Salvador Fernandes Zarco" (p. 318). Pages and pages of Cabalistic symbology are meant to support all this.

This book is filled with unconventional speculation and circumstantial evidence. The author has a vivid imagination. Reading this book is somewhat like reading an espionage novel: if you do not take it seriously, it makes the book fun to read.

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DOUGLAS H. SCHANTZ. *Crautwald and Erasmus: A Study in Humanism and Radical Reform in Sixteenth Century Silesia*. (Bibliotheca Dissidentium, Scripta et Studia, number 4.) Baden-Baden, Germany: Valentin Koerner. 1992. Pp. 254. DM 180.

Even students of the Reformation may be excused if they do not recognize the name of Valentin Crautwald, a colleague of the somewhat better known Spiritualist Caspar Schwenckfeld. Douglas H. Schantz attempts to drag Crautwald out of this undeserved obscurity, but this flawed book may not do Crautwald justice. This is unfortunate because the Spiritualists of the sixteenth century represent a significant link in the evolution of Western religious culture and the secularization of the West.

The first problem with the book is captured in the following statement: "It would appear that the time has arrived for presumptuous statements about 'Schwenckfeld's' doctrines to be revised to give Crautwald proper credit" (p. 146). The reader, and

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Crautwald himself, would have been better served had Schantz adopted a more scholarly tone and purpose. Although most scholars rightly assume a significant contribution on the part of Crautwald both to Schwenckfeld and the Schwenckfelder movement, most of the specific claims made here remain unproven, and in some cases unprovable on the evidence available to us. More damaging are Schantz's often murky and imprecise conceptual categories. On my reading, his discussion of the Schwenckfeld/Crautwald Eucharistic teaching actually corroborates my own evaluation of its origins ("The Genesis of Schwenckfeld's Eucharistic Doctrine," *Archiv für Reformationsgeschichte* 74 [1983], 92–121), an evaluation that he mentions and rejects, but which he never actually engages. The discussion of Crautwald's theory of signs seems not to understand the distinction that I made between a pedagogical and a memorative use of signs (*Caspar Schwenckfeld, Reluctant Radical* [1986], 81). In another place Schantz writes of Crautwald's "typically Spiritualist emphasis upon renewal of individuals through catechizing" (p. 166). Surely catechisms are more often associated with figures like Luther, Calvin, and Canisius?

In fact, Schantz presents Crautwald as a most unconvincing Spiritualist. If we are to accept Schantz's analysis, Crautwald's teaching on the role of preaching and Scripture in the production of faith differs little from Luther's. His discussion of Crautwald's ecclesiology presents it as in tension between a dominant conservative concern for institutions and an ancillary, more radically Spiritualist pessimism. The pattern of citation in the footnotes to that discussion (pp. 178–79, nn. 24–36), however, suggests another possibility: that his conservative statements represent an early period (ca. 1525) and the spiritualistic ones a later period (1534, 1540). Schantz also seems concerned to distance Crautwald from his more radical colleagues. His discussion of Crautwald's apocalyptic thought ranges far to discuss Joachim of Fiore, but it ignores any possible influence of the radical Sebastian Eisenmann, Crautwald's amanuensis. In general Schantz tries to credit Erasmus and Augustine as the sources of Crautwald's thought, a thesis this is not implausible. The result, however, is to make Crautwald a not very interesting figure: a conservative humanist reformer (if Schantz is correct, he was an almost exact contemporary of Erasmus), and not the daring thinker whom Emanuel Hirsch ranked with Luther and Melanchthon as providing the basis of modern thought. Paradoxically, Schantz's evaluation of Crautwald, if correct, would make Schwenckfeld even more interesting and original than has previously been thought. I would be loath to accept that conclusion until further work has been done.

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A. P. Martinich. *The Two Gods of Leviathan: Thomas Hobbes on Religion and Politics*. New York: Cambridge University Press. 1992. Pp. xiv, 430. $59.95.

Standard interpretations of the philosophy of Thomas Hobbes have been flawed by their failure to take seriously Hobbes's strong intellectual commitment to religion. This is the viewpoint that guides A. P. Martinich's book. Martinich claims to remedy this flaw by weaving together elements of both secular and religious interpretations, augmenting that approach with a literal reading of Hobbes. Read literally, Hobbes can be shown to have produced an interpretation of Christian doctrines that reconciles them with modern science and neutralizes their politically destabilizing potential.

Hobbes's theism, Martinich argues, is crucial to the coherence of his political philosophy. Escape from the state of nature depends on the human capacity to enter covenants, and covenanting in a Hobbesian natural condition is impossible without the presence of genuinely deontic laws. Hobbes presents those laws as commands created by God's irresistible power. The result, Martinich says, is a divine command theory of morality.

The problem is to explain how God's power can saturate nature without extinguishing its Hobbesian natural strife. Martinich avoids this revisionism by introducing a distinction between a primary state of nature in which God's power is absent, neither law nor injustice exist, and no covenants are possible, and a secondary state of nature in which God's irresistible power grounds the laws of nature. (It is not obvious that Martinich can introduce this distinction without deserting his commitment to a literal reading.) Because they are roughly equal in power, Martinich argues, people are not sufficiently afraid of others to be motivated to enter covenants in the primary state of nature. Hence the war of each against all. It is the human fear of God, especially God's natural punishments, that makes covenants obligatory in the secondary state of nature. It is not clear, even to Martinich, why natural injuries are perceived as natural punishments, especially since both the disobedient and obedient suffer. God, presumably, can do what God wants because of divine power. Nevertheless, Hobbes's grounding of sovereign authority on divine authority discourages the appeal to religion to justify sedition.

Martinich's study of Hobbes's political philosophy contains several interesting and valuable clarifications of ambiguities. Most noteworthy are his distinctions between renouncing a right and transferring a right, between covenant and contract, and between authorization and authority. Also valuable is his scholarly discussion of the religious controversy that constitutes the historical background of Hobbes's writing.

Following his theistic interpretation of Hobbes's politics, Martinich turns to Hobbes's science, the principal aim of which, Martinich says, was to recon-

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cile Christian doctrine with the principles of modern science. Martinich's argument consists largely of showing that Hobbes's materialist interpretations of heaven, hell, angels, salvation, and miracles are compatible with the materialist and determinist foundations of modern science. It would have been helpful had Martinich given thematic attention to the fundamental project of modern philosophical science to obtain control over nature and erase human vulnerability to what Martinich identifies as natural punishments. Was Hobbes ignorant of the project? Its success would clearly neutralize fear of God's punishments and undermine the authority of God's laws.

Martinich never denies that Hobbes's philosophy does, in effect, undermine the principles of Christian orthodoxy and lead to atheism, only that Hobbes never intended it. Hobbes, he says, was not a subversive, plotting secretly against religion. The traditional view that Hobbes was an atheist who wrote with less than complete candor is rejected as unwarranted. Martinich associates the dissimulation often attributed to Hobbes with intellectual dishonesty and/or intellectual cowardice. He does not discuss the possibility that a nonbeliever might, nevertheless, not want to subvert religion. Lack of candor in philosophical and scientific writing protects religious doctrines no less than it protects philosophical scientists themselves.

This is a useful book. It is the most energetic, and possibly the most rigorous, defense of the theistic interpretation of Hobbes so far. It is sure to be of interest to Hobbes scholars.

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DANIELA GOBETTI. *Private and Public: Individuals, Households, and Body Politic in Locke and Hutcheson.* New York: Routledge. 1992. Pp. viii, 211. $55.00.

In this provocative and complex essay, Daniela Gobetti sets out to illuminate the distinction between private and public as these concepts were put forward in the natural-law theories of John Locke and Francis Hutcheson. Gobetti argues that the prevailing model of the public-private distinction prior to Locke and Hutcheson was inherited from classical Greek philosophy, which maintained that heads of households alone possessed a private domain that distinguished their domestic life from their public life (that is, from their participation in the body politic).

It was Locke and Hutcheson, Gobetti contends, who extended these notions of public and private domains of activity to all adults. Both predicate their natural-law politics on the existence of a state of nature governed by laws that obtain prior to the creation of any political institution. In this natural state, all adults, regardless of sex, are free and equal. All are equally endowed with the rights that are theirs as a consequence of their humanity and, by virtue of these rights, all are regarded as autonomous agents, competent to manage their own affairs. Indeed, natural-law theory at first suggests that even after the advent of civil society one's private domain overwhelms the public, which appears limited to electing representatives and occasionally rebelling against tyrannous governments. Gobetti maintains, however, that the pervasiveness of individual rights, especially in the form put forward by Hutcheson, also extends the area of public participation since "the institutional public may have to intervene in every activity and institution in order to protect each person's endowments" (p. 7).

Up to this point, Gobetti's argument is straightforward. Both Locke and Hutcheson seems to suggest that, to the extent that we pursue our private interests without endangering others' rights, we are acting in our private capacity, while when we defend against and punish wrongdoers, either individually or through the state, we are engaged in a public activity. Gobetti's discussions of the public-private distinction as it emerges in the political philosophy of Locke and Hutcheson are, for the most part, interesting and valuable. It is when she turns to a consideration of the implications natural-law theory has for the nature of political life and for modern notions of empowerment that her argument begins to display fatal weaknesses.

The political dimension, she asserts, even as Locke and Hutcheson conceive it (although they might not have been aware of it), is in fact exceedingly complex. It consists not only of determining who should enforce already existing norms of behavior but also interpreting just what government should do. If we reject this interpretation, Gobetti warns, we are in danger, as was Locke, of marginalizing certain segments of the population such as women and the poor who, if we were to follow a strict interpretation of the laws of nature, would be denied the enjoyment of their private domains. Thus, the relationship between the rich man and the beggar who becomes his servant because he is hungry and that between a man and his prospective bride who accepts a lower position in the family rather than remain single are regarded as harmless by Locke despite the complete asymmetry of power they reflect. Given this weakness in natural-law theory, Gobetti argues, there is something at least mildly salutary in the erosion of the private domain that marks contemporary society.

Gobetti's conclusions suffer from a sometimes tenuous grasp of the distinction between natural and human-made constraints that shape both Locke's and Hutcheson's notions of harm and, in turn, configure their conception of rights. It hardly makes sense to employ the presence of asymmetries of power as a criterion for government intervention, since most private relationships, depending on how one defines "power," are asymmetrical. Indeed, nothing could be more asymmetrical than the relationship between the

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state and the individual. Nor is she particularly sensitive to the more libertarian implications in Locke and Hutcheson, which provide for a society in which individuals may pursue their own ends regardless of whether these ends are socially sanctioned. To suggest that we can counter the centrifugal forces that distinguish twentieth-century society by redefining the political to include any search for socially shared values is to misconstrue both these libertarian implications and the nature of politics itself. Despite these failings, this volume is an interesting and provocative essay that deserves the attention of readers who are concerned with the role these concepts play in seventeenth-century natural-law theory.

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GERALD N. IZENBERG. *Impossible Individuality: Romanticism, Revolution, and the Origins of Modern Selfhood, 1787–1802*. Princeton: Princeton University Press. 1992. Pp. 356. $39.50.

The subject of this book is the politics of Romanticism. It thus belongs in a scholarly tradition reaching back to Irving Babbitt's *Rousseau and Romanticism* (1919) and including the work of Jacques Barzun, M. H. Abrams, and, most recently, Jerome McGann. Gerald N. Izenberg approaches his subject by closely reading the early writings of three important Romantics—Friedrich Schlegel, William Wordsworth, and François-René de Chateaubriand—to assess the logic of their political development, especially as revealed in their responses to the French Revolution. By insisting on the deep affinity between British and Continental figures, Izenberg follows Abrams's magisterial example in *Natural Supernaturalism* (1971), which documented the conceptual links between Romantic poetry and Idealist philosophy, above all between Wordsworth and Hegel.

Izenberg traces the intellectual processes by which Schlegel, Wordsworth, and Chateaubriand became supporters of the French Revolution, only to reject it and embrace political conservatism, moral retrenchment, and traditional religion. The three are among the earliest embodiments of a great modern biographical cliché: the young radical who turns into a reactionary. Their spiritual heirs continue to litter the American intellectual landscape.

Izenberg gives this political transformation a psychoanalytic gloss, anchoring it in oedipal conflicts and incestuous wishes. He views the Romantics' infatuation with the revolution as a revolt against oppressive fathers (or older brothers), abetted by unrealized desires for the mother (or sister), just as their later turn to the right is tied to guilt for having aggressed against the (now often dead) father and for possessing (in fantasy) the mother/sister. Izenberg deploys his Freudian categories skillfully and with a disciplined attention to textual and biographical evidence. His psychological portraits are neither reductive nor mechanical.

The book presents a complex thesis about Romanticism. On the one hand, the Romantics advanced unprecedented claims for subjectivity, marking, as Izenberg's subtitle asserts, the "origins of modern selfhood." On the other hand, their extravagant affirmation of the individual self was forever threatening to turn into its opposite, the subordination of the self to some larger whole (or "totality"), whether another individual (through sexual union), nature, God, the state, the cosmos, or a philosophical abstraction (like "the Absolute"). "The Romantic idea of infinite individuality," Izenberg writes, "is always linked with the notion of an all-inclusive totality other and greater than the self" (p. 8). Beneath the Romantics' specific political commitments (first for, then against, the French Revolution) lay a paradoxical habit of mind whereby the anarchic celebration of self led inexorably to authoritarianism. In its subtle fashion, therefore, Izenberg's book lends support to the argument of Peter Viereck's *Metapolitics* (1941), which located the origins of modern totalitarianism in the Romantic movement.

For all its considerable intelligence, the book suffers from two defects. The lesser is its inattention to literary matters. It treats poems (in the case of Wordsworth) and novels (in the case of Chateaubriand) as if they were no different from expository texts, mining them for their political and philosophical arguments. Of course, ample precedent exists for such an exclusively conceptual approach: one thinks of Lionel Trilling's brilliant analysis of the "Immortality Ode" (1941, reprinted in *The Liberal Imagination*, 1950), which paid virtually no attention to the poem's language or versification. But inevitably a significant part of the evidence gets ignored. It is rather like writing about opera without mentioning the music.

A more damaging weakness is that Izenberg's general thesis concerning the authoritarian logic of Romantic subjectivity tends to drop from view when, in the body of the book, he undertakes his chronological interpretation of individual works by Schlegel, Wordsworth, and Chateaubriand. This is especially the case with the latter two. There is insufficient connection between the book's larger claims and its close readings, which are distinguished by their rigor, sophistication, and engagement with the recent scholarly literature, but which do not clearly bear out Izenberg's general proposition. We are left wondering not only whether the thesis applies to the many Romantics not discussed but also whether it holds even for Schlegel, Wordsworth, and Chateaubriand. As a result, the book will be useful mainly to scholars interested specifically in one of Izenberg's three subjects rather than to those looking for a general discussion of European Romanticism.

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ALAN S. KAHAN. *Aristocratic Liberalism: The Social and Political Thought of Jacob Burckhardt, John Stuart Mill, and Alexis de Tocqueville.* New York: Oxford University Press. 1992. Pp. viii, 228. $39.95.

In this book, Alan S. Kahan takes as his starting point Hans Rosenberg's call in 1930 in *Historische Zeitschrift* for the production of a typology of European liberalisms. Kahan's contribution is not to build a typology of his own, but to bring concreteness and specificity to what he sees as "one of the nebulous shapes of European liberal thought in the nineteenth century, one I here call *aristocratic liberalism*" (p. 3). By defining aristocratic liberalism and relating it to European liberalism as a whole, his hope is to contribute as well to the typological work enjoined by Rosenberg more than sixty years ago.

Who are the aristocratic liberals? They compose a small but distinguished company. Kahan focuses all his attention on just three: Alexis de Tocqueville, John Stuart Mill, and Jacob Burckhardt. These men shared a common overarching commitment to liberty, individuality, and diversity, disliked the materialistic bourgeoisie and revolutionary masses with an equal passion, feared mightily for the future, and pinned whatever hopes they harbored on the humanizing power of education. Their liberalism may be called aristocratic, but only in the literal Greek sense of a belief in the rule of the "best."

Kahan's analysis is comparative throughout. He assigns no individual chapters or even sections of chapters to his three subjects and alludes only occasionally to their quite different lives and careers. Without homogenizing them or ignoring the points on which they clearly disagreed, he prefers to concentrate on their intellectual common ground. In successive topical chapters, he explores their complex reactions to the Enlightenment and the French Revolution, their analysis of their own times, their grounding in a peculiarly modern form of humanism, and their program for a future they all viewed with characteristic anxiety (Burckhardt most keenly, Mill least). In a concluding chapter, he locates aristocratic liberals in the larger flow of liberal thought from 1830 to 1870.

Kahan has done what he set out to do, with relentless lucidity and no observable fear of postmodernist criticism, apart from a disarmingly modest dismissal of the "Foucauldian perspective" in his introduction. Toiling in a field of intellectual history not heavily trampled by such criticism (the history of political thought), Kahan should encounter little trouble on this score. His methods are straightforward. He has ideas about what his aristocratic liberals meant by what they said, he documents his ideas in most instances with ample primary source evidence, and his speculations make good sense. He is also well aware of what else is being written nowadays about European liberalism. One-third of the references in his copious bibliography are to articles and books published since 1980.

Nevertheless, it is possible to quarrel with his vocabulary, his choice of subjects, and the limits he has set for himself. He represents his aristocratic liberals as marginalized free-floating intellectuals, almost Bohemian in Pierre Bourdieu's use of the term. But, as Massimo Salvadori argued some years ago, is it not the case that most liberals were elitists prior to 1870? If so, perhaps "aristocratic" is not the best adjective to separate Tocqueville, Mill, and Burckhardt from their fellow liberals of the period. In any event, Kahan's identification of a separate aristocratic species of liberalism would be stronger if he had discussed at least briefly several other members of the putative tribe. He does name Walter Bagehot and Lord Acton as contenders for the honor in British liberal discourse, but Tocqueville and Burckhardt are left standing in splendid isolation in French and German liberalism.

Finally, why not furnish the reader with at least an educated guess about the other species in the liberal genus? It is really quite difficult to assess the validity of the concept of an aristocratic liberalism without knowing how many other liberalisms of whatever size, shape, and geographical range the author believes were abroad in mid-nineteenth-century Europe. Near the end, Kahan promises to continue his examination of liberalism in a future work, and let us hope that he does. Comparative studies of large movements of thought of the caliber of Kahan's book are all too rare.

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DAVID MACGREGOR. *Hegel, Marx, and the English State.* Boulder, Colo.: Westview. 1992. Pp. x, 345. $44.50.

If Karl Marx boasted that he had turned G. W. F. Hegel right side up, David MacGregor prefers to stay on the right side of both, whom he sees as partners in conceiving an ideal world community. The book begins and ends with Hegel's "political insights" (p. 2) and "vision of the state" (p. 296), as mediated by Marx according to MacGregor. Between beginning and ending, the author, a Canadian sociologist with government experience as an evaluator of social programs, treats his subjects even-handedly in relation to the state. This equilibrium is maintained dexterously by weight of words as against weight of argument.

MacGregor has to grant that "a signal aspect of Marx's (Hegelian) confrontation with the English state is its almost total absence from the bulky literature on both Marx and Hegel." He immediately asks (p. 2), "Am I trying to build a right-wing Marx or a left-wing Hegel?"

Concerning Hegel, MacGregor draws heavily on the philosopher's well-known article, written shortly

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before his death, about issues leading to Great Britain's Reform Bill of 1832. An industrious reader of English newspapers but without any great knowledge of the country, Hegel compared the British government unfavorably to the Prussian state, which he saw as a culmination of the dialectic's movement through history. MacGregor notes how Hegel wielded his contemporary dialectic on English events with dreadful effect, criticizing the projected electoral reforms as insufficient but still radical enough to threaten England with revolution.

If the actually existing Prussian state of Hegel's time approached a conservative ideal, MacGregor raises up his left-wing Hegel to interpret it otherwise: "I will show that Hegel's theory of property is actually an argument for the right of the worker to the whole product of [his or] her labor" (p. 139). Indeed, in this Marxian reversal, "Capital in [Hegel's] vision 'produces' nothing; only the worker has a capacity to create things" (p. 148). MacGregor is not able to "produce" quotations or analyses of Hegelian statements to support this claim, and he ignores Hegel's emphasis on the value of private property as a defense of individual liberty.

Following the strained abstractions of his Marxian Hegel, MacGregor presents a remarkably concrete Hegelianized Marx. In the book's longest chapter, "Marx and the Factory Acts," he recounts how Marx used factory inspectors' reports to document the horrendous working conditions and makes a point of Marx's admiration for Leonard Horner, the chief inspector implementing the acts. The author sees the factory inspectorate as the incorporation of "The Rational State," so entitling his last chapter, where the Hegelian abstractions resume their dominance. Defining Marx as a Hegelian statist, he achieves an authorial epiphany in "the gradualist Hegelian perspective where the bourgeois government is both transcended and preserved—the outlook which emerges most clearly from Marx's treatment of the Factory Acts" (p. 273).

To achieve such a consummation MacGregor had to ignore the evidence, early and late, in Marx's works and life, thus the demand in his "Critique of Hegel's *Philosophy of Right*" (1843) for the pure anarchy of "the *state's dissolution*" (Marx's emphasis), the unqualified call for revolution in the *Communist Manifesto* (1848), the ultimate case for all this in *Capital* (vol. 1, 1867), and his lifetime commitment to a revolution for a stateless world communism. But MacGregor has finessed these components of reality. The collapse of the old Soviet Union marked the end of "something real," he writes in his introduction. "With the disappearance of communism as a reality, the ideal must again take its place" (p. 6). This is a nicely turned *jeu d'esprit*.

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JACK JACOBS. *On Socialists and "the Jewish Question" after Marx*. (Reappraisals in Jewish Social and Intellectual History.) New York: New York University Press. 1992. Pp. xi, 300. $40.00.

Over the past several decades a number of accomplished scholars have addressed the problem of the relationship of European socialists to what became known as "the Jewish Question" in the nineteenth century. Jack Jacobs's treatment of this theme revolves around three central issues: anti-Semitism, assimilationism, and Zionism. For many reasons, beginning with the anti-Semitic rhetoric of Karl Marx himself, "the Jewish Question" has been an especially complex and sensitive issue in the history of socialism in German-speaking countries. Jacobs seeks to confront these complexities by examining the views of several leading German and Austrian socialists, notably Karl Kautsky, Eduard Bernstein, Rosa Luxemburg, Victor Adler, and Otto Bauer. It is understandable that all of these should be included, but unclear why Franz Mehring, who wrote extensively on Jewish matters, is scarcely mentioned. This is only one example of how the title promises more than the book delivers.

Jacobs maintains that "much of the literature on the relationship between socialism and the Jews is marred by political animus or by overgeneralization" (p. 1). He believes that the literature is dominated by essentially two kinds of distortion. On the one hand, a few scholars have argued that from Marx onward all socialists, to one degree or another, looked down on Jews with contempt. On the other hand, other historians exaggerate the extent to which German socialists were free of anti-Semitic sentiments or overstate the unanimity among socialist thinkers on "the Jewish Question." Jacobs sets out to correct these errors, especially the overemphasis on unanimity, and to offer a more differentiated interpretation. His central thesis is that "there was a rainbow of perspectives within the socialist world on the Jewish questions" (pp. 2–3), that "there was no universally accepted socialist or Marxist line on the Jewish question at the end of the nineteenth and beginning of the twentieth centuries" (p. 156). Jacobs's treatment of the leaders he has selected certainly supports this contention, but numerous historians who have written on the subject do not fit into his dualistic classification and he himself has oversimplified the variations in the historiography.

Jacobs's contribution lies, nonetheless, in his careful and detailed exposition of the ideas of the socialists he has chosen. He places great emphasis on the ideas of Kautsky, whose prominence as the leading orthodox Marxist theoretician meant that many people assumed his outspoken rejection of anti-Semitism, his unwavering faith in assimilation, and his polemics against Zionism reflected the attitudes of the vast majority of German Social Democrats. The comparison to Eduard Bernstein, who only as a mature man

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began to think of himself as Jewish, is revealing. By the end of World War I Bernstein's confidence in assimilation had waned, and as it did he showed more understanding for Zionism, which he interpreted as a natural response to rampant anti-Semitism.

Jacobs's chapter on the Austrian socialists, centered on Victor Adler and Otto Bauer, probes the harsh limitations of assimilation in an environment thoroughly poisoned by virulent anti-Semitism. In that context, Austrian socialists, including those of Jewish descent, believed that they sometimes had to take into account the sentiments of anti-Semites and on occasion, for tactical reasons, actually used anti-Semitic arguments against their political opponents. Jacobs rightly concludes that this was a failed tactic that put Austrian socialists on the defensive and undermined their principles and credibility.

Although these detailed expositions of the views of particular socialists are of value, for a fuller and more integrated account of how socialists related to "the Jewish Question" one must still turn to the historians Jacobs believes to be inadequate.

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DAVID F. NOBLE. *A World Without Women: The Christian Clerical Culture of Western Science.* (A Borzoi Book.) New York: Alfred A. Knopf. 1992. Pp. xvii, 329. $25.00.

This is a thesis work. David F. Noble begins by asking why women have appeared so infrequently in the history of Western science and technology. His answer is in the subtitle, namely the Christian clerical domination of science. Noble effectively organizes his data around this theme, and the bulk of the book concentrates on the medieval period. He emphasizes that even though science was increasingly secularized in the eighteenth century; there was little change in attitudes toward women. He notes that Isaac Newton, although not a cleric, continued the hostility toward women expressed by earlier generations of clerics. It was only in the nineteenth century that some change is noted, and Noble explores this primarily in terms of the American experience.

Noble relies heavily on secondary works, not all of them the most authoritative ones, and only occasionally does he list a primary source. The author quite clearly is not a medievalist, but he still seems to have a good grasp of intellectual developments. My most serious criticism, however, is that Noble offers a simplistic answer as to why there were so few women in science and technology in the past. Clerical dominance at best is only a partial answer. Medieval science was heavily dependent on classical science until the thirteenth century, and the view of women in Greek science was not much different than in medieval clerical writing. Moreover, there were almost no women scientists in the classical world or in the Islamic world, nor for that matter were there major figures in the scientific and philosophical writings of India or China. This would lead one to suspect that there is much more to the exclusion of women from science in the West than simple clerical hostility. I would argue, in fact, that in most cultures women were regarded as inferior to males, and the opportunities for them to gain an education were limited. Although women have had somewhat greater freedom at some times in history than at others, even in the best of times their freedom has been restricted, and educational opportunities for them at best were not equal to that of males until the last few decades of the nineteenth century. There were always a few individual exceptions, but to read them is to realize how different they regarded themselves and how many obstacles they had to overcome.

Women obviously had the potential, but societies tended to assign them different roles. In my opinion then, it was not so much clerical hostility to women that was involved but rather general male policy, a thesis I have argued elsewhere (see Vern L. Bullough, Brenda Shelton, and Sarah Slavin, *The Subordinated Sex* [1988]), and nothing Noble says would change my mind.

The changes that have brought women increasingly into the mainstream of intellectual life in the nineteenth and particularly in the last part of the twentieth century therefore have more to do with radical redefinitions of woman's role in society than with any change in the attitudes of scientists. Scientists, in fact, more often played a hostile rather than supporting role in helping women break through barriers, but the growing economic and political power of women has enabled them to overcome such opposition. There is obviously more to the story than this, as my brief overview indicates, but this only emphasizes that Noble's emphasis on clerical culture as the source of hostility is but part of the answer. The clerics were only the spokesmen for what was essentially a dominant male culture that looked on women as wives, mothers, and daughters, but not as citizens, scientists, or intellectuals. It was a man's world in which women had a subordinated role, a role that many women ultimately came to challenge. None of this, however, is recounted in Noble's book. Still, I would recommend that men especially read Noble's book in order to better understand the mind-set with which women have had to contend.

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LYNN HUNT, editor. *Eroticism and the Body Politic.* (Parallax Re-visions of Culture and Society.) Baltimore: Johns Hopkins University Press. 1991. Pp. 242. Cloth $38.50, paper $14.95.

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Lynn Hunt makes two claims for this pioneering cross-disciplinary collection of essays: first, that there is a connection between the erotic and the political, and second, that at its center is "the question of women's place" (p. 4).

The erotic in each of the essays is relatively easy to identify if not to define: a husband nestling dreamily at his wife's breast in the Fragonard painting "Visit to a Wet Nurse" analyzed by Mary Sheriff; women as capital, "not seduced to be consumed but rather to be exchanged" (p. 46), as Anne Deneys argues in her essay on the political economy of the body in Laclos. Sara Maza and Lynn Hunt, respectively, demonstrate how Marie Antoinette's imagined sexual improprieties—her phantasmic rendezvous with Louis de Rohan, bishop of Strasbourg in the Diamond Necklace affair, her even more phantasmic incest with the dauphin for which she was publicly charged and tried in 1793—seemed credible and thus politically potent in a world where women in the public sphere carried with them the scent of disordered sexuality. As Louise de Keralio said—quoted at least twice in the volume—"when a woman becomes a queen, she changes sex."

In two essays eroticism is even more obvious: six aristocratic ladies show their bare buttocks and one her pudendum to the Austrian Army in the 1792 print analyzed by Vivian Cameron; the humiliations of the eponymous heroine of the *Story of Juliette* discussed in Lucinne Frappier Mazur's essay on disorder, ritual, and the social body in the Marquis de Sade are well known. After an interlude of relative chasteness in Deborah Silverman's account of artistic responses to the New Woman in the *fin de siècle*, a "celebration," as she says, of "female fecundity and decorative domestic intimacy" (p. 145), the reader is again on the trail of Eros. Emily Apter is explicitly concerned in her learned essay on female fetishism in Guy de Maupassant to revise "sclerotic psychoanalytic configurations of female eroticism" (p. 183), and Anne Wagner's essay is elaboration of her epigraph from Arthur Symons: "The principle of Rodin's work is sex—a sex aware of itself." Rodin's reality "collapses into a single figure, the captive female body" (p. 229).

Eroticism is clearly everywhere in this collection of uniformly intelligent and thoughtful essays. The problem comes in specifying the political; the connection between the body corporeal—concupiscent, passionate, desiring—and the body politic. Ernest Kantorowicz, whose *Kings' Two Bodies* remains the historiographical model for this enterprise, was profoundly concrete. A doctrine in the political theology of the monarch's body mandates a legal decision regarding a gift of land.

In some of these essays, however, interpretations of the body are license to confuse indeterminacy with politics. Anything that might be read as destabilizing gender is construed as political—generally as politically progressive—on the grounds that political power is often figured as sexual power. Any plausible deconstructive or Bahktinian reading seems to qualify its object as political. This cannot be true. We may all have ceased to believe in the autonomy of the aesthetic, but it does not follow therefore that every text or image does work in the world.

The power of a print like that of women baring their backsides might indeed lie "in its intersection with multiple discourses of eroticism, politics, rituals, Carnival, pornography, reproduction, prostitution and the like" (p. 103). But then again, the print may have no power at all. What is this image actually doing in an ultra-conservative journal, and what are we to make of the embarrassing fact that it appears unchanged in other, politically quite different, venues? To take another example, there is nothing intrinsically political about the lack of "clear and closed meanings" in Fragonard (p. 35). Perhaps libertinism seems at first to subvert the "system of signs" according to which "one says what one feels and does what one says" while a novel about libertinism—*Les Liaison dangereuses*—ultimately "condemns this subversion of natural language, so that it is the law of the signifier that wins out" (p. 57). And perhaps not. Who wins out, where, over whom?

These are questions that can be answered. Maza shows precisely how a set of deeply rooted assumptions about aristocratic, and more particularly queenly sexuality, dictated that Marie Antoinette would be at an embarrassing late night meeting, at least in spirit, if not actually in the flesh. Perverted eroticism was part of the structure of radical critiques of the Old Regime that, in the way of paranoid fantasy, inevitably confirms itself. These confirmations, as Hunt shows, figured directly in revolutionary politics: the queen was tried for teaching her son to masturbate and then having sex with him.

Finally, Anne Wagner cunningly undermines the all-too-obvious political claim that Rodin's art simply "endorses both male mastery of women and the fiction of male sexuality that takes mastery as its premise," while showing that it does that as well. It shares in the popular obsession with female sexuality evident in a journal like *Gil Blas* and at the same time allows a feminist critic like Auriele Mortier to embrace it for "giving her back herself." Wagner's essay powerfully demonstrates the specific political complexity of great art and the impossibility of regarding it as merely ideological.

"The erotic potential of women's bodies" as elaborated in "modern forms of politics, art, literature, and psychology" (p. 13) is certainly central to this collection. In fact, the message seems to be that the female body alone has erotic potential; homoerotic desire figures only, if at all, when triangulated through women. There is nothing wrong with selecting essays that illustrate this point, but then the argument becomes circular. Centrality is demonstrated by the systematic and unexplained exclusion not only of such obvious twentieth-century cases as the unmediated homoeroticism of the *freicorps*, the SA, and much

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fascist male bonding but also of the powerful homoerotics of imperialism, of nineteenth-century schooling, and indeed of much political life down to our own day. The House of Commons was in tears when Charles James Fox and the Duke of Portland split the Whig Party over the question of revolution. When asked by an incredulous foreign observer why Fox maintained any political influence at all, William Pitt replied: "you have not been under the wand of the magician." There too lies Eros.

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ÉRIC BUSSIÈRE. *La France, la Belgique et l'organisation économique de l'Europe, 1918–1935*. (Histoire économique et financière de la France: Études générales.) Paris: Comité pour l'Histoire Économique et Financière de la France. 1992. Pp. 521. 250 fr.

Those who deplore the historical profession's shrinking audience should recognize that many of its practitioners do not know how to write history. They ignore their audience; omit identification, context, and explanation; regurgitate data in numbing detail; and eschew conclusions. Thus, much valuable research remains unread. Dissertations are particularly prone to these defects and usually need major revision before publication. That of Éric Bussière escaped any firm editing.

This is a pity. In the first half of this century, Belgium's economic and strategic importance far exceeded its size, yet its role is rarely studied except by Belgian scholars whose vision and research are often narrow. Economic relations were a key aspect of France's dealings with Belgium, with which it conducted in 1923–24 a joint foreign policy of economic and financial significance. A bilateral study therefore is amply justified.

Bussière, who is French, did the necessary research, working extensively in both countries in government archives, papers of statesmen, and records of banks and industrial concerns. He recognizes that Belgium was a separate country with its own interests, and his sweep is broad, from tariffs and Luxemburg through the Ruhr occupation to the steel cartel and financial crisis, the economic conference of 1927, electricity, the Bank of International Settlements, and the gold bloc.

Unfortunately, this material is organized on the principle of contemporaneity, with the result that one story is usually interrupted by an installment of another, and elements that belong together are scattered. The work has more longitudinality than breadth or depth; it is crammed with data, but adheres narrowly to economic and financial matters with little regard for political and strategic factors even when these determine policy, as was largely the case in French policy toward Belgium. Curiously, Bussière treats the two countries as equals, ignoring the disparity in power, and he never fully faces up to French determination to render Belgium a satellite. Thus we learn of Étienne Clémentel's and Louis Loucheur's schemes for an economic accord without Bussière mentioning that they were variant approaches to the same goal.

The book is addressed to the handful of experts who can supply such context and explanations; identify persons and organizations; wade through a thicket of acronyms and awkward, allusive, arid prose, often in the present tense; and cope with a slipshod scholarly apparatus. This last includes a pitifully incomplete abbreviation list, an inadequate index (not always clarifying the acronyms), and a bibliography omitting works cited in the text. Even experts will be dismayed by the omission of findings of recent scholarship, carelessness about titles of books and names of individuals, authors, and organizations, and the lack of precision in page citations and reporting from monographs.

Despite his research, Bussière depends heavily on secondary works on France, without much regard to reliability. He does better by Belgium, perhaps because there is less literature and he writes from the records. About a quarter of the book treats Franco-Belgian economic relations, narrowly defined and circumscribed. The remainder relates Belgium's external economic and financial history, even more narrowly circumscribed and entirely from the Belgian viewpoint. This is so distorting that Émile Francqui becomes the prime author of the Dawes Plan and the Bank for International Settlements, without mention of Owen Young or Montagu Norman, while the Belgo-German mark agreement of 1930 far outstrips the Young Plan in importance. Those unfamiliar with the Dawes Plan, the London conference of 1924, or the reparations reprise in 1929–30 will learn neither the basic context nor French policy goals for those events.

Alas, this work never addresses fundamental questions about relative power in various forms, security, and national independence. It is also contradictory: a Franco-Belgian economic accord "could have succeeded" (p. 456), but it was an unnatural combination of two states with nothing in common save hostility to German economic power. Greater Franco-Belgian economic solidarity occurred, however, in eras of economic crisis, without a link to German power, economic or otherwise. Bussière's conclusion is mostly recapitulation, but he does argue that the early 1920s was a period of French bilateralism, the later 1920s an era of multilateralism occasioned by German reentry into the economic scheme of things, and the 1930s a decade when Belgium pressed for regional groupings that were precursors of broader postwar European schemes. Here is food for thought. One only wishes that Bussière had rendered the road

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to it less bumpy and more accessible, especially to a wider audience.

SALLY MARKS
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PAUL MOJZES. *Religious Liberty in Eastern Europe and the USSR: Before and After the Great Transformation.* (East European Monographs, number 337.) Boulder, Colo.: East European Monographs; distributed by Columbia University Press, New York. 1992. Pp. xviii, 473. $56.00.

This informative but uneven volume surveys the state of religious liberty, or its absence, in communist Eastern and Central Europe, and considers the prospects for widening religious freedom in the wake of the recent political upheavals in that region. Paul Mojzes absolves the founders of Marxism of blame for the religious persecution invariably practiced by communist states; while hostile to religion, Karl Marx and Friedrich Engels assumed it would wither naturally in the light of socialism. Vladimir Lenin and the Bolsheviks decisively committed their regime to war against religion and its adherents, a battle raised to new levels of cruel intensity by Joseph Stalin. Devoting separate chapters to the experiences in each of nine countries, Mojzes stresses that all communist governments formed in the Soviet shadow copied the Bolshevik formula for treatment of religious questions no matter how inappropriate such policies were to their own circumstances. Following the "Great Transformation" of 1989 and after, the former Soviet bloc made genuine if imperfect progress toward religious freedom, despite the disquieting threats posed by chauvinism and intolerance.

Mojzes bases his study on a variety of published materials in several languages, supplemented by numerous personal interviews. He alerts the reader that he intends to focus on the issue of religious liberty rather than the more conventional topic of church-state relations, but in fact no one, Mojzes included, can avoid the role of temporal power in assessing religion under communism. His conclusions are well taken for the most part, and he presents his findings with such evident knowledge and conviction that one hesitates to dwell on the shortcomings that diminish his work. An unacceptable number of editorial errors mar the text, and the narrative suffers in parts from the author's brave attempt to keep pace with the furious pace of recent events; sections dealing with post-communist developments give the appearance of being hastily inserted in the interests of currency. The lack of an index presents a serious obstacle, especially for those interested in tracing particular topics or confessions throughout the region rather than in following the book's country-by-country organization.

Mojzes couches his discussion in a theoretical and typological jargon of his own construction that often produces opacity, but other readers may find this style less frustrating. From time to time one encounters questionable assertions or key passages in which Mojzes fails to make his meaning plain. To cite but one example, while faulting the communists for the deplorable recent history of religious liberty in Eastern Europe, he adds that: "It is also obvious that religious people wittingly or unwittingly contributed to the calamity both prior to . . . and during the Communist period" (p. 395). The statement is defensible, it is intriguing, but it is by no means obvious; offering no further explanation, Mojzes misses the opportunity to drive home a point he clearly regards as vital. In sum, readers will profitably consult this volume, but it gives more the feel of a good rough draft than a finished book.

NEAL PEASE
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ROBERT C. ALLEN. *Enclosure and the Yeoman: The Agricultural Development of the South Midlands, 1450–1850.* New York: Clarendon Press of Oxford University Press. 1992. Pp. xiv, 376. $79.00.

This is a wonderful book. Robert C. Allen has compiled a number of large and carefully controlled data sets on landholding, agrarian production, employment, and income. He uses these to confront the three tenets of what he calls agrarian fundamentalism: that enclosures and land consolidation were necessary to the technical revolution in English farming; that England's uniquely high agricultural productivity provided the capital, labor, and cheap food necessary to launch the industrial revolution; and that the English and all later agricultural revolutions required a drastic increase in inequality. Both Tories and Marxists subscribe to these beliefs, the former believing that dispossessed peasants actually gained from becoming wage laborers, while Marxists contend that landlords received the lion's share of benefits from capitalist agriculture.

Allen investigates each of these claims in both their Tory and Marxist variants. His central finding is that there were two agricultural revolutions in England. During the late sixteenth and seventeenth centuries a yeomen's revolution led to the introduction of new seed varieties and labor-intensive innovations in farming techniques, accounting for almost 90 percent of the total increase in per-acre yields in England from 1450 to 1850. The eighteenth-century landlords' revolution, leading to the consolidation of farms, raised yields significantly only on heavy arable lands where capital-intensive hollow drainage was installed. Large farms cut back significantly on labor, especially reducing the employment of women and children, thereby further raising productivity per worker. It was in that dimension that English agriculture surged ahead of farms elsewhere in northwest-

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ern Europe with similar per-acre yields. Most of the dismissed agricultural workers, however, did not find employment in eighteenth-century protoindustries, becoming instead the legions of poor.

Allen's data and analysis provide little comfort for the once-again fashionable Tory fundamentalist view. Consolidation of farms did not add appreciably to the supply of food, nor did commodity prices decline in the eighteenth century. Small farms had generated capital and labor for improvement through the savings and efforts of yeomen owners. Commercial farmers often lacked the capital to improve large farms. Landlords, the main receivers of surplus, tended to spend their income and mortgage their land for conspicuous consumption rather than investing in new industries or improving their own estates.

Allen also contributes to our understanding of the mechanisms of agrarian change in England. The Marxist model receives only limited support from Allen's data. Enclosure was the principal means of dispossessing peasants in the sixteenth century, operating much as Marxists have described. During the eighteenth century enclosure generally worked by agreement. Landlords got rid of small tenants by letting their leases expire. Although the process of consolidation was different from what Marxists believe, the consequences were unfortunate for all but landowners; even commercial farmers lost ground after 1650. Whereas prices, wages, and return on agricultural capital were roughly stable or declining from 1450 through 1850, real rents to landholders multiplied at least sevenfold over those centuries. Once tenants lost their long-term leases, only landlords benefited from gains in productivity created in the main by yeomen farmers. Grand houses rather than industry were the main artifacts of landlord hegemony.

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T. E. HARTLEY. *Elizabeth's Parliaments: Queen, Lords and Commons, 1559–1601*. New York: Manchester University Press; distributed by St. Martin's. 1992. Pp. 184. $59.95.

The serious study of Queen Elizabeth's Parliaments began in the 1950s, when J. E. Neale published his two-volume work, *Elizabeth I and Her Parliaments* (1953–57). For two decades Neale's account, which was initially greatly respected, remained unchallenged. During the 1970s my own monographs about the Parliaments of Henry VIII raised questions, at least by implication, regarding Neale's failure to include the House of Lords in his narrative and his excessive concentration on supposedly controversial bills, to the exclusion of much important but more routine legislation. These concerns came to a head in 1986 with the publication of G. R. Elton's book *The Parliament of England 1559–1581*, a meticulous reworking of the ground covered in Neale's first volume, highly critical of both Neale's approach and his scholarship. Meanwhile, Norman L. Jones had rejected Neale's reconstruction of events in Elizabeth's first Parliament, which was based more on intuition than solid evidence, while David Dean and J. D. Alsop had published articles correcting or amplifying other aspects of Neale's work.

One of Neale's disciples, chosen by him to edit and publish the private diaries on which Neale relied so heavily in his narratives, was T. E. Hartley. In the present volume Hartley enters the fray, summarizing the present state of knowledge on a number of topics and suggesting his own resolution of some issues under debate.

As might be expected, Hartley positions himself somewhere between Neale and Elton. He is quite willing to criticize his mentor. In particular, he accepts the revisionist view that the so-called "Puritan choir" was a figment of Neale's imagination, and he concurs with the related conclusion that Neale exaggerated the extent to which Puritans were organized to lobby for religious change. He agrees that Neale overstated the extent to which the queen and the Commons opposed each other, although he is reluctant to accept the alternative view that cooperation generally characterized Parliament's sessions. He acknowledges Neale's weakness in overlooking the Lords but argues that it is justified, at least in part, by the absence of private diaries and other sorts of evidence that would enable the historian to accord it equal consideration. He devotes a chapter to Peter Wentworth, whom Neale pictured as a Puritan leader who locked horns with Elizabeth over the issue of free speech: Wentworth's religious views are in fact ambiguous, but he was certainly not the chief spokesman for the reformers, and his imprisonment in the Tower may have been based on the fact that he improperly "leaked" information about parliamentary proceedings rather than on his eloquent call for freedom in debate.

Hartley does little to rectify Neale's greatest oversights. Despite his subtitle, which appears to recognize the importance of the Lords, he does not really increase our knowledge of the upper house. Materials are in fact available for such a study. The Lords Journals, although brief and formal, include a daily record of attendance, which we lack for the Commons; the manuscript original acts preserved at the House of Lords Records Office contain evidence about committees and amendments that can occasionally be quite revealing; the fact that the Lords (unlike the Commons) could give proxies casts light on friendships and political affiliations within the chamber. Similarly, Hartley follows Neale in ignoring huge categories of bills and acts. He has virtually nothing on economic and social legislation, private acts, or abortive proposals. His one attempt to rewrite the history of a single session, that of 1601, reads much

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like Neale's version, with no reference to the poor law that provided the basis for poor relief until the nineteenth century.

What we need now, instead of more interpretive essays, are solid monographs on the Lords and on economic and social legislation. When this work is complete someone should start over and produce a new account of what went on in Elizabeth's Parliaments, working from the actual sources rather than becoming enmeshed in the crossfire of historiographical debate.

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JAMES S. HART. *Justice upon Petition: The House of Lords and the Reformation of Justice, 1621–1675*. London: HarperCollins. 1991. Pp. 287. $65.00.

The revival of impeachment proceedings against royal officials by the House of Lords in the 1620s was the tip of the iceberg in the Lords' resurrection as a court of both first instance and final appeal in seventeenth-century England. The revival of impeachment received attention from a number of scholars in the 1960s and 1970s, but, with the exception of Elizabeth Read Foster's study, *The House of Lords, 1603–1649* (1983), only cursory note was taken of the Lords' resumption of other forms of jurisdiction. James S. Hart, plunging into the archival waters of the House of Lords Records Office, has now surveyed the iceberg's hitherto submerged mass.

As Hart notes, the resumption of impeachment by the Lords in 1621 coincided with the revival of their appellate jurisdiction over royal courts. The occasion for the former was corruption and abuse by crown officials, for the latter, the dysfunction of the court system generally. Hart treats these as separate lines of inquiry, but it can hardly be regarded as accidental that the most prominent victim of impeachment in the Parliament of 1621, Sir Francis Bacon, was also the chief legal officer of the realm and the head of its most embattled court, the Chancery.

The Lords owed the efficacy of their judicial revival to the happenstance of frequent Parliaments in the 1620s, which made petitioners see them as a viable resort and enabled them to maintain a reasonable degree of continuity. If the irregular sittings and abrupt dismissals of the High Court of Parliament made it less predictably accessible than the courts of Westminster, its dispatch of business compared favorably to theirs, and its judgment, when rendered, was authoritative. Hart gives the Lords high marks for judicial probity, praising the political disinterestedness of their determinations and their concern to maintain proper jurisdictional boundaries both among courts and between themselves and the court system as a whole. He points out that the justices attended and advised the Lords, as did members of the king's Privy Council (many of whom were peers themselves). In effect, he asserts; the Lords functioned as a general board of review, bringing together the best expertise from the executive and judicial realms.

This view is unconvincing. There was precious little harmony between crown and Parliament in the 1620s, and, as Conrad Russell has shown in his *Parliaments and English Politics, 1621–1629* (1979), the upper house was often a cockpit of factional struggle among councilors. The fact, noted by Hart but subsequently ignored, that a number of the selfsame councilors—a Lord Chancellor in 1621, a Lord Treasurer in 1624, a Lord Admiral in 1626—were objects of impeachment as well hardly comports with the image of a body dispensing impartial and politically neutral justice. Moreover, the Lords failed signally to discharge the task advertised in Hart's subtitle, "The Reformation of Justice." Insofar as Parliament was impelled to resort to impeachment as a means of restraining the crown, and to judicature to redress the defects of the courts, it pointed to a functional breakdown of government. In the latter case the appropriate remedy was legislation, not judicial activism. Despite discussion over five decades, that remedy was not forthcoming.

The abolition of the prerogative courts and the virtual paralysis of the executive in 1640–42 thrust a wholly unmanageable judicial burden on the House of Lords; the civil war left its remaining members in an increasingly untenable position, culminating in the dissolution of the house in 1649. Hart portrays the Lords as striving "to maintain order and preserve the public peace" (p. 166), as if these were politically neutral goals in a time of political and social revolution (or at any other time, for that matter), and scolds them for losing objectivity only in the later 1640s (pp. 199 and following). Similarly, he sees the renewed flood of petitions to the Lords in 1660 as evidence of public faith in their impartiality, although the vast majority of them came, unsurprisingly, from ejected ministers and owners of sequestered estates. And it is only in Hart's discussion of *Skinner v. East India Company* and *Shirley v. Fagg* in the Cavalier Parliament that he seriously engages the chronic rivalry between the Lords and Commons over judicature, or indeed takes much notice of the Commons' share in it.

The full story of the revival of judicature in seventeenth-century Parliaments still awaits its historian. That story will include its political context, the dispute between the Commons and the Lords over its control, and its impact on the two great revolutions it so importantly shaped. Hart is to be commended for extending considerably our knowledge of technical procedure in the Lords, for tabulating and analyzing its caseload, and for a number of revealing case summaries. He offers shrewd and suggestive observations on the workings of seventeenth-century English law. But the self-imposed limitations of an insti-

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tutional study need not have so severely constricted his vision. The Lords' iceberg, after all, rammed and sank a ship of state. That is surely as significant as the legal debris it carried on its journey.

ROBERT ZALLER
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WILLIAM PALMER. *The Political Career of Oliver St. John, 1637–1649*. Newark: University of Delaware Press. 1993. Pp. 153. $29.50.

This first book-length study of Oliver St. John describes the political career of a pivotal figure of the English Revolution. Born of a gentry family, St. John was educated at Cambridge and Lincoln's Inn, becoming a prominent lawyer. In 1627 he was hauled before the Star Chamber, an experience he never forgot. But William Palmer argues that the critical event in shaping St. John's attitudes was the Ship Money case of 1637, in which he defended John Hampden. His defeat—by a far closer margin than some have suggested—convinced St. John of the dangers of royal autocracy, fears that the outbreak of the Scots rebellion and the Papist Plot confirmed. He became a leader of the Long Parliament and engineered the execution of the king's chief adviser, the Earl of Strafford, whom he described as "vermin."

St. John saw himself as defending an ancient constitution even if that meant going to war with the king. Herein lies the dilemma that eventually destroyed him. Like the American officer in Vietnam who explained after an air strike that "we had to destroy the village in order to save it," St. John was forced into a more radical, illegal, and even unconstitutional position than he would have preferred in order to rescue his conservative vision of the law and ancient constitution.

After John Pym died in 1643, St. John became the parliamentary leader who persuaded the Scots to invade England in order to save England. He removed the Earl of Essex, who had been chosen more for his aristocratic pedigree than his military skill, from command of the parliamentary army. Yet the vacuum that he created was filled by Oliver Cromwell, a radical who destroyed the legal and constitutional system St. John desperately wanted to preserve.

And thus St. John made himself yet one more of the English Revolution's many victims. As Palmer astutely observes, he was "often ideologically conservative, but operationally liberal" (p. 115). Although he refused to take part in Charles I's trial, and thus escaped the regicides' bloody fates after the Restoration, he was forced into exile, dying in Balse in 1673.

Palmer has written a good study of this rather enigmatic figure. Based on his dissertation, it is soundly grounded on all the appropriate archival and printed sources. The view he presents about the origins and nature of the English Revolution—that it was essentially a legal and constitutional struggle—is curiously old-fashioned. This, however, is not surprising, for St. John was, at heart, a lawyer and constitutionalist. He was also an extremely private man about whom very few personal details have survived. Nonetheless, Palmer is at times a little hesitant about his conclusions. Modifiers such as "it seems" and "it appears" speckle the pages of this short book. Indeed, had he tried to write a full-scale biography and not a political study of a dozen, albeit crucial, years, we might have gotten a fuller treatment and more confident portrait of an important and hitherto neglected man.

CHARLES CARLTON
North Carolina State University

BARRY COWARD. *Oliver Cromwell*. (Profiles in Power.) New York: Longman. 1991. Pp. vii, 204.

BERNARD COTTRET. *Cromwell*. Paris: Fayard. 1992. Pp. 542. 160 fr.

To the writing of books on Oliver Cromwell there is, it seems, no end. Both of these books, however, are useful additions to our understanding of the man who bestrode the English Revolution like a colossus.

Bernard Cottret's biography is almost three times the length of Barry Coward's, and is much more heavily weighted toward the 1640s; half of Coward's book is devoted to the 1650s. Yet there are many areas of agreement and similarity between the two works. Both authors see Cromwell as a man of contradictions, of "ideological schizophrenia," in Blair Worden's phrase (Coward, p. 2). Religious idealist and political pragmatist, scourge of Parliaments who never ceased striving for parliamentary rule, radical reformer who also sought "healing and settling," superb practitioner of realpolitik who yearned for Jesus Christ to "set up his reign" in men's hearts (Coward, pp. 100, 109), Cromwell continues to perplex and fascinate both authors.

The authors have assimilated the most recent research on their subject by Worden, John Morrill, John Adamson, Peter Gaunt, Austin Woolrych, and others, although Coward's command of the material is surer than Cottret's. They agree that Cromwell, even during his five years of political supremacy, was no dictator in the modern sense. Although he could purge and dissolve Parliament at will, he could not, even when he assumed the mantle of Lord Protector, veto its legislation, and thus he had to submit to repeated reductions in his military budget. Nor did this "dictator with a human face" (Cottret, p. 384) rule by fiat between Parliaments. Rather, he followed the path of close consultation with the sixteen or so men who made up his Protectoral Council.

Cottret and Coward recognize Cromwell's agonized response to the offer of the crown in 1657 as the supreme crisis of his career. His rejection of the offer arose not out of detestation for the institution of monarchy; not even out of fear of what his comrades-

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in-arms would say if he accepted the crown. Instead, it was because of what God would think. Providence, he reasoned, had already witnessed against monarchy. To accept the crown would be to defy that witness and thereby to jeopardize the cause of godly reformation.

As Coward rightly insists, providence was one of the watchwords of Cromwell's whole career; the other was necessity. Together they explain why he emerged as the driving force behind regicide in 1649, expelled the Rump Parliament in 1653, and declined the crown in 1657. In his penetrating analysis of the 1650s, Coward rejects the conventional view that Cromwell became steadily more conservative as he grew older and that he died a "broken man" (p. 154). He remained a religious radical until his dying day. He yearned for peace and social stability, but his overriding goal was godly reformation. This entailed not only the suppression of drunkenness, sexual immorality, and cruel sports but also the reform of education, government, and the law, and an attack on poverty. It also meant converting the nation to Christianity while allowing people the liberty, within limits, to interpret that experience in their own way.

Cromwell's great failure was his inability to advance this central objective of his career. In his own mind his other achievements—rendering republican government acceptable to the majority of the English people, maintaining social order and stability, and making England respected and feared on the international stage—would have paled into insignificance. But his most positive legacy, according to Coward, was the establishment of Protestant Nonconformity as a permanent feature of life in Britain, leaving "an indelible religious imprint on the development of his country" (p. 176).

Only a few errors mar Coward's sound and judicious account. It was not the defunct General Council of the army but the Council of Officers that debated and approved Henry Ireton's *Remonstrance of the Army* in November 1648; only one mutineer was executed in the aftermath of the Ware mutiny; and the *House of Lords Journal* does not record that Viscount Saye and Sele introduced the Self-Denying Ordinance in the upper house on December 9, 1644. Coward is perhaps also too ready to accept Mark Kishlansky's denial (*The Rise of the New Model Army* [1979], 205) of the Levellers' role in politicizing the army in 1647.

Cottret's book is by his own acknowledgment more literary and "metaphysical." He reminds us that Cromwell has often but erroneously been compared to Napoleon, and that Voltaire, Jean-Jacques Rousseau, Victor Hugo, and François Guizot have had interesting things to say about him. Another bonus is a number of salient quotations from the "Correspondance Politique" in the Ministère des Affaires Extérieures in Paris, although they add nothing essential to our knowledge. Cottret places heavy emphasis on Cromwell's hypocrisy, duplicity, and Machiavellianism, and he displays impatience with Cromwell's Calvinist doctrine of predestinarianism, dismissed sardonically as "this wholesome theology" (p. 206). Perhaps his greatest originality is in drawing our attention to the religious similarity of Cromwell and Charles I. Each man claimed liberty of conscience for himself; each was willing to brave death for his religion; and each had a sense of being chosen by God.

Like Coward, Cottret is guilty of a few errors. John Lambert was not a regicide; John Beard, Cromwell's schoolmaster, far from being a Puritan inspiration was, as John Morrill has shown (*Oliver Cromwell and the English Revolution* [1990], 27), a greedy pluralist; anti-Catholicism is trivialized by comparing it to the contemporary witch craze; and the apocryphal story is repeated that Cromwell would have gone into exile had the Grand Remonstrance not passed.

In summary, both books provide satisfactory accounts of Cromwell's achievement, except that the military dimension of his genius is given perfunctory treatment. Coward's account will be preferred by students, not merely because it is written in English, but because of its conciseness, reliable judgments, and up-to-date command of Cromwell scholarship.

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Lawrence Stone. *Uncertain Unions: Marriage in England 1660–1753*. New York: Oxford University Press. 1992. Pp. xiii, 281. $35.00.

With its raunchy tales and vigorous prose, and the elaboration of a bold thesis uncontaminated by the contrary arguments of recent scholarship, this book is vintage Lawrence Stone. The work forms the second volume of Stone's trilogy on marriage and divorce. It comprises a series of case studies, in the main based on documents from the court of arches, about the ambiguities and uncertainties surrounding the making of marriage between 1660 and 1753. The studies are interlaced with provocative comments and prefaced by an introduction explaining the relevant legal, cultural, and social background. The stated central concerns are to illustrate the significant changes in marriage practices that occurred in this period, most notably the increasing reluctance of the courts to uphold a clandestine marriage; and to uncover from recorded behavior the moral attitudes toward courtship and irregular marriage. Both of these, however, are secondary to the author's desire to regale the reader with stories and, in so doing, to bring the past alive, a vital part, he feels, of the historian's craft.

The sources he relies on portray a sexually promiscuous, intensely avaricious world in which "high and low society appeared for a time to have lost their moral bearings . . . Everyone was busy chasing everyone, in hot pursuit of money, sex, or power" (p. 134).

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Predatory males, scheming females, sex-starved widows (do we really need the replay of early modern stereotypes in current scholarship?), forgers, and perjurers abound. The main conclusions Stone presents are that for the period 1675–1710, relations between the sexes seem to have been at their most brutal; many propertied families experienced an acute economic crisis; bundling was widespread; and the clandestine marriage system concealed a great deal of bigamy.

Stone's interpretations are not always persuasive. For example, he claims that "As a result of glaring defects in the laws of marriage, very large numbers of perfectly respectable people in the seventeenth and eighteenth centuries could never be quite sure whether they were married or not" (p. 3). On the contrary, the evidence collected here clearly shows that people knew of the law on marriage and at times either wished to get around it or manipulate it to their own advantage. The case studies highlight the extent of the doubts entertained by people with regard to anything less than the prescribed marriage service. Those who underwent clandestine marriage ceremonies were well aware that they were on shaky legal ground and took steps to try and rectify this by making sure they had witnesses to the transaction and/or a certificate.

The women in this book are frequently the mercenary ones, or the promiscuous ones. Men are the dupes. Well, maybe. Or maybe it was not the "voracious" sexual appetite of women but their necessity for maintenance that led to the taking of lovers. Nevertheless, the examples Stone cites expose the vulnerability of women. Although a few women were successful in playing the marriage game, most were not. Clandestine marriage was a much riskier undertaking for women than men since women incurred harsher social penalties for sexual lapses than men. Women invariably did not remarry if the illicit union was dissolved, whereas men did.

The vignettes, however, do disclose a murky underworld, one inhabited by the propertied as much as by the propertyless. They reveal the remarkable degree of sexual freedom permitted by some members of the middling and upper ranks. Long-lasting sexual relationships without any kind of marriage ceremony were not uncommon. The documents also highlight the active role of women in marriage matters. Young women would initiate courtships and mothers could foster the illicit romance, often without the knowledge of the father. Surprisingly, mothers were not necessarily the guardians of their daughters' morals but could permit fondling in their presence and arrange for suitors to spend the night in an adjoining room. That the individuals who turn up in these cases were people of a reasonable standing is clear; whether they would also be regarded as typical or even respectable by their peers is much less certain.

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ALAN CRAIG HOUSTON. *Algernon Sidney and the Republican Heritage in England and America*. Princeton: Princeton University Press. 1991. Pp. 335. $39.50.

JONATHAN SCOTT. *Algernon Sidney and the Restoration Crisis, 1677–1683*. (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1991. Pp. xv, 388. $69.50.

These two fine books examine the life, times, and political thought of Algernon Sidney. Through his careful analysis of Sidney's thought, Alan Craig Houston, a political theorist, effectively questions some central maxims of the republican historiography that developed from the pioneering works of J. G. A. Pocock, Bernard Bailyn, and Gordon Wood into the currently dominant paradigm for interpreting seventeenth-century English and eighteenth-century American political thought. Jonathan Scott, a historian, in this second of a two-volume study of Sidney, brilliantly illuminates the essential, complex interrelationships between biography, political history, and political theory; in the process, he offers a useful lesson in the methodology of historical interpretation as well as a convincing reinterpretation of the Restoration crisis of 1677–83, a critical period in the most critical century of English history.

Houston's book (based on an award-winning doctoral dissertation) aims primarily at contemporary debates over the nature and significance of republicanism. Enthusiasm for the "republican revival" among historians and political theorists has been tempered, on the one hand by evidence "that classical republicanism was far less important than Lockean liberalism to the intellectual life of modern America," and on the other hand by the realization that "the cultivation of civic virtue is an unattractive and impractical ideal for large, complex, and heterogeneous societies" (pp. 3–4). Classical virtue, Houston reminds us, is "a martial ethos that manifests itself in a cult of masculinity and in a glorification of conquest and expansion" (p. 9).

In any event, Houston believes that "despite the enormous attention that republicanism has received, its specific character has frequently eluded contemporary analysts" (p. 4). We need to know more, for instance, about the precise meaning of virtue—in republican discourse it sometimes has multiple and inconsistent meanings—and about the allegedly hostile relationship between the republican language of virtue and the liberal language of rights. Insofar as "Sidney represents the essence of republicanism in England and America," an analysis of his political writings will "shed new and revealing light" not only on Sidney himself—a victim of neglect by twentieth-century historians and political theorists alike—but also, and more importantly, "on these problems in contemporary analyses of republicanism" (p. 4).

Houston shows that republican revivalists who cite Sidney as the perfect historical foil to the Lockean liberal tradition in America have severely underesti-

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mated the complexity and profundity of his thought. Their Locke is a straw man and their Sidney is an icon; neither villain nor hero have much substance. Yet these thinkers explode the antithetical paradigms we invent and impose on them for explanatory or ideological purposes.

For example, contemporary scholars have insisted that "the defining characteristic of republicanism is a classical theory of virtue, and that the republican language of virtue is distinct from and in tension with the liberal logic of rights and interests" (p. 146). Yet as Houston lucidly demonstrates, a distinctive feature of Sidney's thought consists not in any advocacy of civic virtue as an alternative to liberal rights and interests, but in a "dramatic reconfiguration" of classical republican ideas and the fusion of these transformed themes with liberal theory (p. 157). For Sidney, "civic virtue does not grow out of the renunciation of private interests, but rather out of the recognition that the vast majority of private interests are encompassed in, indeed are part and parcel of, the public interest . . . [S]elf-interest was the strongest possible foundation for civic virtue" (p. 166). In Sidney's republicanism, "the language of virtue and corruption was inseparable from the [liberal] logic of rights, interests, laws, and contracts" (p. 169). Hence there is reason to doubt "the widely held view that there existed a distinct and coherent 'republican' language of politics in Revolutionary America that was distinct from and in tension with Lockean liberalism" (pp. 224–25). This argument, extended to a range of themes, indicates "the need to modify the conceptual framework employed by republican revivalists" (pp. 9, 277).

Scott's book lacks the neatness and organizational unity of Houston's study. Scott himself calls it "unruly," "ungainly," and "rough-hewn," flaws attributable to the inherent difficulty of straddling "the two substantially separate disciplines of intellectual and political history" (pp. xii, xiii, xv). Yet it is a stunning achievement. Scott delivers an almost tangible Sidney, and he creates a dramatic sense of immediacy about the personal and political crises of Sidney's turbulent life. Scott's Sidney is more of a philosopher, but no less a man of action—even violent action—than the Sidney depicted by Houston. And whereas Houston studies Sidney's political thought largely to contribute to contemporary analyses of republicanism, Scott explores the connections between Sidney's thought and its political context in order to illuminate both.

Scott seeks to reconstruct that context from the perspective of its contemporaries. This is the methodological point of departure for a major reinterpretation of the Restoration crisis. Historians tend to interpret this period through the prism of subsequent events, so that it becomes "artificially wedded to its future and artificially severed from its past." As a result, the removal of James II from the throne in 1688 has led historians to inflate the importance of the attempt to exclude him from the succession to the throne a decade earlier. Yet contemporaries, Scott reminds us, interpreted their situation not as prophets of what would come, but as "prisoners of memory" and as "a generation living uniquely under the shadow of its past." As such, they viewed the events of 1677–83 as a "repeat screening" of 1638–42, a "second Stuart crisis of popery and arbitrary government" inextricably tied to the European Counter Reformation. In this context, the contemporaries, including Sidney himself, viewed "the exclusion proposal" as "one result of this crisis, not a cause of it," and not even as a primary issue (pp. xi, xiii, 3–9).

Scott's treatment of Sidney is no less superb than his reconstruction of the Restoration crisis. Like Houston, he presents a "historiographic rebel": a thinker who "does his level best to resist the historian's glib hunger for categorization" (p. 351). Reflecting his century, his family life, and the high politics in which he engaged, the essence of Sidney's political thought is struggle. "Conflict stands at the heart of Sidney's experience, whichever way we look at it" (p. 349). His republicanism, for example, is based on a vision of the human condition as a struggle to lessen the distance between man and God through the acquisition of knowledge and political action. Absolute monarchy, especially as developed by Robert Filmer, and indeed the monarchic principle itself increase that distance from God by denying people the liberty and therewith "the chance to improve themselves, and condemns them to an eternity of wickedness and sin" (p. 219). The moral obligation to replace monarchic with republican government—by armed insurrection if necessary—is therefore inalienable. (Sidney, a veteran of the Commonwealth, lost his head because he explicitly called for and actively conspired to bring about a second republican insurrection in England.) Republican government, moreover, is active government, the principal agent of the progressive change that is always vital to the well-being of political society.

Sidney emerges from both of these excellent studies justifiably revitalized, by substantial research and cogent analysis, as a principled (although imperfect) man of action and a serious thinker. Houston, through Sidney, scores a direct hit against the republican revival; Scott's work—poignant and epigrammatic—promises to become authoritative. Each book, in its own way, makes a valuable contribution that will enrich the discourse for years to come.

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MOIRA FERGUSON. *Subject to Others: British Women Writers and Colonial Slavery, 1670–1834*. New York: Routledge. 1992. Pp. xiii, 465. Cloth $55.00, paper $19.95.

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Moira Ferguson's admirable study of the images and paradigms embedded in the antislavery stance of women writers is a tribute to the rich rewards of combining literary and historical studies. Linking the history of slavery and antislavery resistance to the appearance of literary texts, she identifies the vital role women writers played in the construction of understandings of "white" and "black," colonizer and colonized. Whereas historians of antislavery movements have noted the prominent part played by women, Ferguson fills in the picture of how women writers developed definitions of themselves against the "other" provided by colonial slaves, self-definitions that ultimately inspired them to speak out against their own subjugation in later reform efforts. Wrestling with the need for liberty and rights for both slaves and women, their impact on the images of slaves available to Europeans and Americans fell far short of true liberation.

The principal objective of this book, to resuscitate the female discourses of antislavery, uncovers several important themes within the history of abolitionist arguments. Christianity, Englishness, and whiteness worked in tandem to empower the antislavery advocate, male and female. But what gave the female perspective such trenchancy? The evocation of a certain form of femaleness was evident in most critiques of slavery. A crucial link connected the antislavery impulse and traits culturally defined as feminine, particularly as these qualities emerged in the eighteenth century in evangelical religion and in charity and reform movements. Sympathy, feeling, compassion, love for others—quintessential female virtues—were necessary for the identification of the slave as worthy of salvation. Ferguson would have strengthened her case by examining "feminized" male authors, since many nineteenth-century religious men claimed the same qualities within a masculinized missionary realm. (Henry Whiteley, for example, produced *Three Months in Jamaica* [1833], the best-selling antislavery pamphlet.)

Ferguson identifies a second theme, the revolutionary rejection of subjection, through the spiritual leveling of seventeenth-century female Quakers, the political leveling of eighteenth-century women radicals, and the argument against gender inequalities by nineteenth-century feminists. She brings to light voluminous evidence of poetry and fiction by women that explored the relationships between master and slave, colonizer and colonized, and, ultimately, male and female.

The third and most forceful theme within Ferguson's analysis is the final reinscription of unequal power relations between colonizer and colonized within women's writings. Using Aphra Behn's *Oroonoko* (1688) as a paradigm, she points to the paradoxical result of so much imaginative pondering of the condition of African slaves: the denigration of the African, who is by definition destitute of all those characteristics deemed human by Englishmen. Ferguson's discussion of a sole exception, Mary Prince's narrated autobiography, published in 1831, is perhaps the climax of the book, offering a moving analysis of an ex-slave's incredible struggle for survival.

This summary hardly does justice to the complexity of Ferguson's readings or the issues at hand. One hopes that this kind of study will hasten an end to the sexual division of labor that has characterized the history of the antislavery movement to date. In a similar vein, one hopes that no account of British thought leading up to the 1830s will overlook the role of race in constructing British images of power and authority.

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F. C. MATHER. *High Church Prophet: Bishop Samuel Horsley (1733–1806) and the Caroline Tradition in the Later Georgian Church.* New York: Clarendon Press of Oxford University Press. 1992. Pp. viii, 333. $76.00.

F. C. Mather's biography of Samuel Horsley, whom Mather describes as the "ablest ecclesiastical statesman of the later eighteenth century" (p. vi), fleshes out the thesis of his important article, "Georgian Churchmanship Reconsidered: Some Variations in Anglican Public Worship 1714–1830" (*Journal of Ecclesiastical History* [1986]). Like Jonathan Clark's revisionist study, *English Society 1688–1832* (1985), Mather's work challenges the received tradition concerning the Whig and latitudinarian dominance of English religion and society during the eighteenth century.

In response to Norman Sykes's portrayal of the latitudinarian hegemony over the Church of England (*Church and State in England in the XVIIIth Century* [1934]), Mather affirms the persistence and continued strength of the Catholic and sacramental piety of High Church Anglicans in the eighteenth century. Although Sykes may have rehabilitated the eighteenth-century church, and especially the episcopate, from the charge of negligence, Mather accuses Sykes of neglecting High Church contributions to church and society, thus replacing "one myth by another" (p. 2).

Horsley's life story provides Mather with a context in which to explore the emphases of High Church tradition that emerged among the Caroline divines of the seventeenth century and were passed on through a variety of mediators, including John Johnson and John Hutchinson to High Churchmen of Horsley's generation. Mather outlines five points that defined High Church piety at the end of the eighteenth century: quarrels with Methodists over polity and with Calvinist enthusiasm; a reaffirmation of Catholic teaching on the Eucharist; conflict over proposed revisions of the prayer book; defense of fundamental doctrines of the church such as the Trinity and the

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incarnation of Christ; and defense of the establishment of the church.

Mather traces Horsley's life from his middle-class origins as the son of a clergyman through his education at Cambridge and Oxford and early years as a parish priest to his years in the episcopate. A protégé of the latitudinarian bishop Robert Lowth, a distinguished member of the Royal Society, and editor of the works of Isaac Newton, Horsley began to exhibit High Church tendencies by the late 1780s. These tendencies were first seen in his strong response to Joseph Priestly's publications on the nature of the Trinity, and Horsley's elevation to the episcopate in 1788 provided the proper forum for defending High Church interests. As a member of the House of Lords he espoused the idea of divine-right monarchy in the face of the French Revolution, opposed the repeal of the Test and Corporation Acts, and yet he supported the abolition of the slave trade. He also offered a sympathetic ear to Roman Catholics and Scottish Episcopalians and sought to bring reform to the parishes of his diocese, especially with regard to the lingering problem of clerical nonresidence.

Although Mather has provided an important contribution to a long-neglected field, some questions are left unanswered. Why, for instance, was Horsley more committed to the principle of state establishment than to apostolic succession, as evidenced in his ambivalent relationship to the Scottish Episcopal church? Although Horsley was definitely an Erastian, greater attention needs to be given to the contrast between Horsley's position and that of High Churchmen earlier in the century. The picture might have been clearer if Mather had devoted greater space to Horsley's theological assumptions concerning the nature of the church and episcopal succession. Still, Mather's work is a needed and valuable addition to our knowledge of the period.

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L. G. Mitchell. *Charles James Fox*. New York: Oxford University Press. 1992. Pp. 338. $49.95.

L. G. Mitchell has written a distinguished biography of Charles James Fox, easily the best to date. His first two chapters, on Fox's career to 1782, are brilliant and gracefully written essays. They argue that Fox was not rooted in firm principles during this period but was guided instead principally by family loyalty, self-interest, and personal friendship. The middle chapters, often repetitious and less well written, argue that Fox's political views—what Mitchell terms his "Whiggery" (p. 71)—were fundamentally and permanently formed by the events of 1782–84. His subsequent political behavior was entirely governed by his passionate desire to drive home the constitutional lessons of those years. By 1797, when he seceded from Parliament, there was "no fight left in him. George III had quite simply won" (p. 145). Fox had been marginalized by the French Revolution, and he knew it. His lifelong preference for a nonpolitical life among his friends came strongly to the fore.

Mitchell's interpretation is a sound one as far as it goes. Fox's late integration into the Rockinghams is well presented, and the influence of Edmund Burke on his political views is suitably described as secondary to his experience of royal influence in 1782–84.

Mitchell correctly notes that Fox was wholly unconcerned with the administration of party machinery, rarely formulated policy, and was disastrously inept at informing and unifying his supporters in times of crisis. His "refusal to lead" (p. 72)—indeed his continuing ambivalence toward political life—allowed divisions and animosities to develop in the party well before the French Revolution finally split it. The Fox that emerges from Mitchell's biography is more fascinating than he has ever been. His private life is especially well portrayed. But he is not the traditional figure of party myth. In his two concluding chapters, among the best in the book, Mitchell explicitly denies that Fox was the founder of nineteenth-century Liberal reform.

It is a pity that so fine a book must also have major deficiencies. The Duke of Portland, dismissed as "not the cleverest of men" (p. 52), is almost entirely ignored. Instead Fox is presented as leader of the party, which is repeatedly termed "Foxite," even in the late 1780s. This is a fundamental error. Although Fox enjoyed a uniquely prominent position and was leader in the lower house, from July 1782 until the party split more than a decade later Portland was the undisputed—and very active—leader of the party. Mitchell also seems to remain unaware of much centrally important evidence in the Blair Adam manuscripts, especially on the precise role of Lord North in the party leadership after 1783 and on the Fox subscription. His unnecessarily myopic focus on Fox himself also leads Mitchell to slight the key ideological developments that were being introduced by Charles Grey and his friends in the early 1790s. It was these which were to contribute to nineteenth-century Liberalism. Although Mitchell treated such matters in earlier book (*Charles James Fox and the Disintegration of the Whig Party 1782–1794* [1971]), the present biography lacks balance and can be seriously misleading to the uninitiated. Nevertheless, it is a fine work and most welcome.

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F. Rosen. *Bentham, Byron, and Greece: Constitutionalism, Nationalism, and Early Liberal Political Thought*. New York: Clarendon Press of Oxford University Press. 1992. Pp. xii, 332. $76.00.

The relationship between abstract political theory and concrete historical practice remains an issue of

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formidable complexity. In this important monograph, F. Rosen explores the often illusory connections between Jeremy Bentham's constitutional theories and the British political activists who invoked his name while supporting the Greek struggle for independence during the 1820s.

Rosen divides his study into three parts. In "Theory," he analyzes the complicated evolution of Bentham's constitutional philosophy from the early *Fragment on Government* (1776) to the more mature reflections of the *Constitutional Code* (1830). As in his earlier work, Rosen draws on an intimate knowledge of the voluminous Bentham papers to reveal nuances missed by earlier commentators. He emphasizes the critical role of liberty in Bentham's constitutional theory and shows how his radical notion of popular sovereignty undermined the traditional separation of powers but roughly coincided with his famous utilitarian principle.

From one perspective, Bentham's eagerness to apply his theoretical insights to the movement for Greek independence during the 1820s proved unsuccessful. His advice was welcomed but ignored. Yet as Rosen demonstrates in his discussion of "Ideology," Bentham's influence assumed a different form. Philhellenes invoked both his name and a simplified, often distorted version of his philosophy to help legitimize the Greek struggle. Although now forgotten, Edward Blaquiere garnered wide praise from Greek patriots for his active support of national self-determination, a principle Blaquiere claimed descended from Bentham. Another important "disciple," Leicester Stanhope, constantly cited Bentham to buttress an authoritarian liberalism only tangentially related to its proclaimed source. Indeed, Rosen argues that Blaquiere and Stanhope, rather than the more celebrated Bentham and Byron, shaped the ideological spectrum of the philhellenes during the 1820s. Within the tangled web of Whig, radical, and liberal ideologies, myth often proved more potent than reality.

Nowhere was this confusion more disheartening than with Lord Byron, whose active partisanship and eventual martyrdom buttressed the legitimacy of Greek nationalism immeasurably. Yet as Rosen demonstrates in detailed chapters on the London Greek Committee, the activities of the philhellenes often provided a lethal mixture of confused ideology and inept ambition. Byron died squalidly in a botched military intervention. Over a million pounds of financial support for the movement was squandered in scandal and corruption, in part because liberal philhellenes opposed contributing the money directly to the Greeks themselves. As icons of liberalism, both Bentham and Byron professed views more complicated than and often in conflict with the ideology and practical activities they inspired in others.

Although Rosen's general point about the disjunctures between theory and practice ought not be startling, his detailed case study explores courageously and with admirable empirical vigor one of the most difficult problems in the history of ideas. To be sure, Rosen's rigorously thematic organization often results in repetition, as chronology is sacrificed to analytical categories. But Rosen grasps the complexities of influence in ways that traditional intellectual history often ignores. In the process, he contributes not only to a more thorough and rigorous understanding of philhellenism in Britain but also to the complex development of liberal ideology in the early nineteenth century.

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Wendy Hinde. *Catholic Emancipation: A Shake to Men's Minds*. Cambridge, Mass.: Blackwell. 1992. Pp. 211.

Catholic emancipation has long been regarded as the more important of the two reforms of church and state effected in the final few years of the Tories' long hold on office. Although repeal of the Test and Corporations Act was first in the trinity of constitutional reforms culminating in the great Reform Act, it was the troublesome Irish issue that had dominated British polity. Tied as it was to popery and the wretched Irish peasantry, emancipation was anathema to the Protestant soul of England, and its passage took its subjects into uncharted waters.

Observers then and since have noted the irony that the Duke of Wellington's antireformist government should introduce, and carry, such a measure. Ultra-Tories who had rejoiced at the exit of the liberal Canningites from government were to find themselves confounded by the series of events leading to Catholic relief. How this came about forms the subject of Wendy Hinde's study. Relying mainly on printed sources and some archival evidence, Hinde describes the passage of the measure through cabinet and Parliament. Following a short introduction of the issue, and a description of English and Irish Protestant fears of the movement led by Daniel O'Connell, she sets out in a series of chronological chapters the details of the well-known story.

Hinde's contention that Wellington alone possessed both the determination and prestige to carry a bill is perhaps correct, but there was surely more to the matter than that. Emancipation had twice before in the 1820s carried the House of Commons, and although Hinde implies, without much evidence, that it was the duke who overcame a reluctant peerage and king, O'Connell's bold election in County Clare and the fear of insurrection in Ireland were two essential ingredients missing at the time of the earlier Commons' successes.

Readers looking for a novel interpretation or an examination of the issue based on hitherto unavailable evidence will not find such here. Rather lengthy descriptions of cabinet formations and wrangling, of parliamentary debates and newspaper reactions, all

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matters described in greater detail elsewhere, substitute for sustained analysis such as promised on the dust-jacket blurb. Hinde's point that rich Catholics like O'Connell did not understand the economic plight of the peasant could well have led to a useful discussion of the merits of emancipation; Wellington and Peel's reluctant concession in order to preserve aristocratic power was in sharp contrast to their views on parliamentary reform. Unfortunately, analysis of emancipation in the context of parliamentary reform, declining monarchical power, and the absence of peace and prosperity in Ireland is limited to a few paragraphs at the end. If there is need for a further study of emancipation, that need has not been met.

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STANLEY CHAPMAN. *Merchant Enterprise in Britain: From the Industrial Revolution to World War I.* New York: Cambridge University Press. 1992. Pp. xvi, 339.

Stanley Chapman's central argument is that historians of economic growth and social change have missed the crucial role that the merchant entrepreneur played in the century and a half of economic growth that began in Britain with the Industrial Revolution. Concentrating on merchant entrepreneurs engaged in foreign commerce as wholesale traders, Chapman, a historian with many previous books on economic development, has written a pioneering exploration into the remarkable (and largely uncharted) response of merchants and "international" merchant houses to the unprecedented opportunities of the period from later eighteenth century through the Victorian age.

Over this century and a half, there was forward and backward economic integration as merchants moved into manufacture and manufacturers moved into the merchant trade. Particularly after the second half of the eighteenth century, merchants found the chance of profit in manufacturing difficult to resist. As a result, merchants developed a new form of business organization, the "agency house" (or "commission merchants," later known as merchant banks or "accepting houses"). Encouraged by British imperial expansion, merchant "agency houses" were international in character and did much speculating (particularly in the Far East) not only as traders but also as investors in manufacturing and promoters of new industries (such as distilling and the teak trade). Chapman claims that by 1890 the agency houses were so successful that the entire textile industry was under their domination. Even more significant, merchants came to dominate about two-thirds of the entire export trade of Britain, and with idle capital at hand, they became major investors in manufacturing. Such was the case with the Scottish merchant house of Jardine, Skinner & Co., which came to control six of the twenty-one jute mill companies in India (in 1890) and which invested over £40,000 in indigo factories in 1846.

Chapman has much to say about the nature of British entrepreneurship, particularly the migration of foreign (largely Jewish, German, and Irish-American) merchant families to London and provincial cities and how this rich ethnic and cultural mélange provided needed investment for the north of England. In telling this story, Chapman challenges the thesis (set forth by Martin J. Wiener and others) that British—usually provincial—industrialists suffered discrimination at the hands of an aristocracy of London's gentlemen merchant-financiers. On the contrary, the essential feature of the mercantile community in London and elsewhere was not its exclusiveness but its ethnic, cultural, and organizational diversity. Furthermore, Chapman suggests that the standard claim that British merchants wisely concentrated on trade within the empire may be true, but such concentration did not necessarily mean that the British were more or less successful in those areas than non-British firms.

Overall, Chapman has done a brilliant job of opening up new perspectives onto many old playing fields of economic and business historians: the history of entrepreneurship, trade and empire, and capital formation. Unfortunately, he too often tells us what did not occur rather than what did occur. The book can be (and most certainly will be) challenged on a variety of fronts. The author's reliance on Liverpool and Glasgow (and Manchester and Bradford) for supporting evidence, for example, makes one wonder why he suggests the primacy of London, and in general the questions raised are stronger and more attractive than the evidence presented. Nevertheless, the reader cannot but conclude that the merchant community greatly stimulated foreign demand for British goods and, as well, its inventive nature is the reason it survived longer than its counterpart in other Western economies.

JAMES SCHMIECHEN
Central Michigan University

BRUCE L. KINZER *et al. A Moralist In and Out of Parliament: John Stuart Mill at Westminster, 1865–1868.* Buffalo, N.Y.: University of Toronto Press. 1992. Pp. viii, 317. $65.00.

John Stuart Mill was the leading political thinker of Victorian Britain; consequently, the history of his tenure in Parliament will interest intellectual and political historians alike. In this excellent book, Bruce L. Kinzer, Ann P. Robson, and John M. Robson provide the definitive treatment of Mill's three years in parliamentary politics. The volume is clearly written and based on extraordinarily thorough research. Its quality comes as no surprise, because all three authors are experienced Mill scholars, and John Robson is editor of Mill's collected works.

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The authors set the elections of 1865 and 1868 (in which Mill won, then lost, a seat from Westminster) as bookends around chapters on each of Mill's principal political interests at the time: political and constitutional reform, women's suffrage, Ireland, and the Governor Edward Eyre controversy. Taking their cue from Mill himself, the authors ask why Mill was elected in 1865 and why, once having been elected, he was defeated in 1868. Their account, while surprisingly including no economic or social analysis of Westminster, offers a very full and plausible political narrative. They conclude that in 1865 Mill's standing as a political philosopher, his forthright advocacy of unconventional views, and his electoral aloofness (he refused to help finance his campaign, to attend to local constituency problems, or to canvass the voters) all contributed to his reputation as a man of probity and distinction. But Mill failed to see that the constituency was shifting to become a safe Conservative seat by 1868, and his defense of several unpopular causes made him seem an extremist.

The authors eschew a chronological account of Mill's three years in the House in favor of a thematic analysis of his ideas and action on four clusters of issues. Their strategy is largely successful, especially in regard to women's suffrage and the Eyre controversy. What is striking about Mill's role in the reform crisis of 1866–67 is how consistently he supported William Gladstone, not out of blind loyalty but because he judged Gladstone to be the best chance to radicalize the Liberal Party. As for women's suffrage, the authors claim Mill knew his proposal would be defeated, but that he fulfilled his intention of putting the issue before the public in a way that avoided ridicule. Their analysis of Mill's views on Irish land, university, and constitutional issues is well-informed and sympathetic to Mill, but it seems too wedded to Mill's own outlook and therefore lacks detachment. In the matter of Eyre, the authors show that Mill had made up his mind on Eyre's guilt before the judicial procedures were completed; hence, Mill's determination to have Eyre punished revealed Mill for once in an intemperate and vengeful turn of mind.

The authors also take up the question of Mill's effectiveness as a House of Commons representative. They conclude that despite an ill-considered maiden speech on Ireland, Mill earned the respect of other members and raised the tone of debates. He was not a good speaker because he suffered from nervousness and a thin voice. But with his conscientiousness and supremely cool reasoning powers, he eventually gained attention on set issues. Moreover, he did not enter the House as a political innocent, for he had long taken an interest in parliamentary affairs and served for many years in the East India Company. He was, after all, not an ivory-tower academic, but "a shrewd man, an idealist but a shrewd idealist" (p. 20).

This is a fine, well-integrated book. The authors do not really demonstrate that Mill was thinking and acting as a moralist in his parliamentary career. They do show, however, that Mill self-consciously advocated political positions ahead of conventional opinion because he felt his role as a thinker required him to take the lead on advanced issues and then step aside when his views had earned a central place in mainstream politics. Moreover, they show that Mill deliberately promoted the radicalization of the Liberal Party. Mill assumed progress in public affairs was the result of the enlightenment and persuasion of the political community by intellectual leaders. He stood for a seat in Parliament because he felt doing so would enable him to put his ideas to a broader public than his writings had reached, but he accepted defeat with equanimity because he felt that after all he might be able to promote his objectives better outside Parliament than in it.

THOMAS WILLIAM HEYCK
Northwestern University

BRUCE C. WESTRATE. *The Arab Bureau: British Policy in the Middle East.* University Park: Pennsylvania State University Press. 1992. Pp. xvi, 240. $35.00.

Long enshrouded in mystique and the shadow of Lawrence of Arabia, the Arab Bureau, situated in Cairo's Continental-Savoy Hotel (formerly New Hotel), has been a puzzlement. Until Bruce C. Westrate's perceptive book, every student of the Middle East knew of the Arab Bureau, but no one really knew what it did. Perhaps this is as it should be in the nebulous world of an intelligence agency, but it is not the stuff of which history is made. To the Viceroy of India and his elegant and elaborate staff, the sleuths in Cairo were blundering bazaar manipulators trying to encroach on the turf of the government of India in the Persian Gulf, the Red Sea, and the Middle East. To many in London the Arab Bureau represented a bunch of muddle-headed academics making policy that rightly belonged to them. Others dismissed the Arab Bureau as an Oriental establishment dazzled by the prospect of a united Arab state under the hegemony of Husayn ibn 'Ali, King of the Hejaz. All of these themes appeared true and virtuous, if not a trifle sanctimonious, when Lowell Thomas discovered T. E. Lawrence in Arabia and proceeded to make him a national hero (a status Lawrence confirmed in his *Seven Pillars of Wisdom*, and David Lean enshrined in his epic motion picture *Lawrence of Arabia* [1964]). To his credit, and our relief and gratitude, Westrate not only places Lawrence in the perspective from which history should remember him but also clearly demonstrates that none of the above was true.

Established in Cairo in 1916, the Arab Bureau was a collection of hard-headed Arabists led by David George Hogarth (an archaeologist and former keeper of the Ashmolean Museum in Oxford, fluent in French, German, Italian, Greek, Turkish, and Arabic) and the indomitable and unflappable Gilbert "Bertie" Clayton (who commanded an extraordinary

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group of men, the least of whom, one could argue, was Lawrence). The purpose of the Arab Bureau was to instigate a revolt among disgruntled Arabs, particularly the Bedouin of the Arabian peninsula, and to destroy Turkish control over the Arabic-speaking Middle East.

It was never their intention to poach on the imperial sphere of the government of India; they had enough to do in Cairo and the Hejaz. Nor were they amateurs at intelligence. To be sure, most were academics, but they knew Arabic and the Arabs as experts in London and New Delhi did not. Clayton was probably the most remarkable exception, knowing Arabic and the Arabs better than anyone, but trained at Woolwich as a military engineer. None were incompetent, as is frequently implied, and none were unaware of the larger imperial objectives of British policy in the Middle East. If anyone is to blame for the disasters that beset British policy after World War I, it was more the incompetence of professionals like Mark Sykes in London and General Archibald Murray in Cairo.

Finally, Westrate has laid to rest the canard, long applied to the Arab Bureau, that they were a conglomerate of pro-Arab dilettantes dreaming of a united Arab state in the Middle East. Nothing could be further from the truth. Hogarth and Clayton were realists who knew the Arabs well enough to recognize that such a state was an unlikely proposition.

This is a first-rate piece of detective work, putting the story straight by methodical scrutiny of the history of British intelligence in the Middle East in World War I. John Le Carré would be pleased.

ROBERT O. COLLINS
University of California, Santa Barbara

TIM TRAVERS. *How the War Was Won: Command and Technology in the British Army on the Western Front, 1917–1918*. New York: Routledge. 1992. Pp. xviii, 232. $59.95.

Few victorious armies have had their commanders damned so heartily and so often as the British Army in France in World War I. In an earlier book titled *The Killing Ground* (1987), Tim Travers explained how the army's continued adherence to prewar military doctrines contributed to the pursuit of futile offensives and horrendous casualties. In the present work, an extension of this earlier analysis, he argues that despite the continuation of many weaknesses in its command structure and probably at a cost higher than necessary, the British high command did respond sufficiently to the situation in 1918 to play the key role in the Allied victory.

Travers's conclusions are based on a detailed analysis of the British command structure during critical phases of the last fifteen months of the war. He demonstrates that the crucial weakness of this structure into the spring of 1918 continued to be a rigidly hierarchical system in which all authority went from the top down, inhibiting both the flow of essential information in the reverse direction and the necessary coordination at the division, corps, and army levels. He uses this insight to shed new light on the battles at Passchendaele and Cambrai in 1917, but he applies it most significantly to explain how the German successes in the spring of 1918 were due not only to their innovative infiltration tactics but also to those structural weaknesses that prevented the British command from responding effectively to the conditions of mobile war that ensued. When the Germans reverted to traditional methods of massed assaults, however, British firepower inflicted heavy casualties, which led ultimately to the German defeat by July 1918. The successful British offensives that followed were characterized initially by employment of tanks, but thereafter primarily by infantry-centered attacks supported by overwhelming artillery superiority. Coupled with a somewhat more decentralized command structure that gave greater authority to experienced army and corps commanders, these tactics produced a steady advance against the already defeated German army until the armistice, but one that was nonetheless extremely costly.

Solidly based on primary sources and an astute understanding of the techniques and problems of military operations on the western front, Travers's work is a brilliant command analysis that can serve as a model for future studies of any army or period. His conclusions will please neither the most ardent defenders nor critics of British generalship but are well reasoned and convincing. That he has fully explained how the war was won is less certain, however. His conclusion that the German defeat came in July rather than the fall leads him to ignore the French or American contributions to the final Allied successes, but his admission that the British suffered over three hundred thousand casualties during this period seriously weakens this claim. Defeated armies simply do not inflict casualties of this magnitude on their opponents.

Nonetheless, this book is an outstanding contribution to the literature on the British Army in World War I and essential to any serious collection.

ROBERT H. LARSON
Lycoming College

F. H. HINSLEY and C. A. G. SIMKINS. *British Intelligence in the Second World War*. Volume 4, *Security and Counter-Intelligence*. (History of the Second World War.) New York: Cambridge University Press. 1990. Pp. xii, 408. $42.95.

This volume, with the companion study, *Strategic Deception* by Michael Howard (1990), completes a unique and meritorious venture in official history. Earlier volumes in the series, appearing seriatim from 1979, dealt with intelligence and operations in the

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European theater. The main story was the origin and contribution of Ultra, the code name for intelligence derived from decoding German radio communications. Ultra was, it is now safe to say, a war-winner.

The history of security and counterintelligence lacks the consistent drama of Ultra, and F. H. Hinsley and C. A. G. Simkins have some trouble imposing a coherent structure on their material. The text is dense with the code names of double agents, the measures taken to sustain them, and the details of the eternal bureaucratic battles fought out between MI5 and MI6. This volume also suffers from the unexplained decision to separate the treatments of counterintelligence and deception. As Hinsley and Simkins show, the authorities had effective control of the situation on the home front relatively early in the conflict. Increasingly, their counterintelligence product was utilized to support an aggressive series of deception plans culminating in the Fortitude North and South operations of 1944, designed to camouflage the Normandy invasion. Security and counterintelligence during World War II were not insignificant matters; it is simply hard to see them in isolation from the rest of the intelligence effort.

Hinsley and Simkins are convincing in their summation of the principal reasons for British success in counterintelligence: the stimulus afforded by the invasion scares of the summer of 1940; the rampant inefficiency of their main enemy, the *Abwehr* (German secret service); and the performance of signals intelligence. The authors see this last factor as the real key. The material code named ISOS (Intelligence Service Oliver Strachey), drawn from *Abwehr* messages, allowed the British to survey with confidence the whole field of Axis espionage activities and to neutralize or control and play back enemy agents. The Double-Cross system, which fed misleading intelligence from controlled agents to the Third Reich, was one of the most imaginative and high-risk enterprises of World War II. It would have been impossible without ISOS. The statistics are impressive: ISOS decoding totaled some 268,000 messages during the war, with the peak daily output of 282 reached in May 1944. Equally impressive was the fact that by this date British counterintelligence had more to worry about from the activities of freelance operatives like "Ostro" in Lisbon, who ran "paper mills" that churned out quantities of fictitious intelligence based on guesswork, guidebooks, and the daily press for the unsuspecting *Abwehr*. Ostro, it was feared, might just invent the truth.

This volume, like the rest of the official history, is an essential reference source. Not the least of its services is that it pricks popular mythologies about spying while leaving plenty of scope for future enquiry. One puzzle remains, however. It is surprising that nothing is said about the infamous "Cambridge Five" (the Communist spy ring of Kim Philby, Guy Burgess, Donald Maclean, Anthony Blunt, and John Cairncross), who must be regarded collectively as one of the security disasters of the war. Mythologies will undoubtedly continue to circulate about the activities of these and other "moles."

WESLEY K. WARK
University of Toronto

FRANK CALLANAN. *The Parnell Split, 1890–91*. (Irish Studies.) Syracuse, N.Y.: Syracuse University Press. 1992. Pp. xxiv, 327. Cloth $42.50, paper $19.95.

Frank Callanan's book is the most significant study in Irish history to appear since the publication of Roy Foster's *Modern Ireland 1600–1972* (1988). Seeking to recast F. S. L. Lyons's and Conor Cruise O'Brien's interpretation of Charles Stewart Parnell's last campaign as a desperate gamble, Callanan argues impressively, if not totally convincingly, for a basic consistency on the Irish leader's behalf. With an exhaustive use of primary sources, and an especially thorough examination of the local Irish press, Callanan focuses on Parnell's efforts to regain control of the political debate in the aftermath of the sordid O'Shea divorce.

Parnell was outmaneuvered by former lieutenants like Tim Healy, men whom he had raised to prominence in the early 1880s. Callanan shows how Parnell failed to see how the Home Rule Party, which he had largely created, had dramatically changed by 1890. Peasant proprietorship, a commitment of the Catholic clergy to Home Rule, and the rise of a shopkeeping petit bourgeoisie had transformed the Irish political scene and doomed Parnell's chances of regaining control. In a sense, Parnell was out of touch with the changes that had taken place in his own party since 1886. Although Callanan argues that Parnell's efforts were not a "hubristic error of judgment" (p. 16), he would have been wise to follow Cecil Rhodes's advice: "resign, retire, return." That was temperamentally beyond Parnell.

While Callanan defends Parnell's actions as rational during the struggle to reclaim control of the Home Rule Party, in my view the viciousness of his attacks on William Gladstone personally and on the Liberal alliance reveal an uncharacteristic recklessness and loss of balance. More tragically, Callanan shows that the exchange of slanders by Parnell and his foes "offered to English opinion a welcome restoration of a reassuringly comic vision of Ireland . . . which Parnellism had banished" (p. 73). The decisive intervention of the Catholic hierarchy against Parnell also revived old English misgivings of ecclesiastical domination of Irish affairs. The ramifications of the Parnell split not only doomed him personally but also shaped the contours of Irish politics for the next thirty years.

Callanan's brilliant reconstruction of 1890–91 shows how the debate between Parnell and his enemies, especially Healy, developed a logic of its own from which neither man could escape. Both sides in the split were victims, but Ireland was the real loser.

Callanan's chapter "The Political Economy of Parnellism" is his weakest and reads like an afterthought.

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It argues that Parnell's economic views were coherent and sophisticated. In fact, they sound like ideas thrown together in desperation as a way of separating Parnell from his opponents. Aside from that caveat, Callanan's study ranks with the best work done in recent years as a must read for any scholar of modern Irish history.

JOHN P. ROSSI
La Salle University

MARYANN GIALANELLA VALIULIS. *Portrait of a Revolutionary: General Richard Mulcahy and the Founding of the Irish Free State*. Lexington: University Press of Kentucky. 1992. Pp. xii, 289. $35.00.

Building on her earlier work (*Almost a Rebellion: The Irish Army Mutiny of 1924* [1985]), Maryann Gialanella Valiulis has produced a fine study of a man whose crucial role in Ireland's revolutionary struggle for national independence and in the preservation of constitutionalism in the fledgling Irish Free State has been largely obscured by historians focusing on revolutionary leaders like Michael Collins, Arthur Griffiths, and Eamon de Valera.

This richly detailed portrait, based on an exhaustive examination of public and private collections and interviews with Mulcahy's contemporaries, sheds considerable light on the complex interaction of the military and political goals that motivated the Irish revolutionary elite and, hence, of the causes of the Irish civil war.

Although Mulcahy was elected as a Sinn Fein representative in December 1918 and held political office in the first revolutionary Dail government, this study concentrates primarily on his military career from his enlistment in the newly formed Irish Volunteers in 1913 to his imaginative and effective leadership as chief of staff of the Irish Republican Army (IRA). A detailed analysis of Mulcahy's attitudes and actions as chief of staff of the Irish Free State Army and eventually as minister of defense provides significant commentary on the controversial beginnings of the Irish Free State.

Mulcahy participated in the Easter Rebellion of 1916 and served time in Frognoch prison, experiences that steeled his determination to fight for Irish self-determination and his concern that this struggle be executed with a minimum of bloodshed. As chief of staff of the Volunteers after 1918, he eschewed large-scale military initiatives and encouraged the use of small, mobile units known as flying columns. Thus, Mulcahy was influential in transforming the Volunteers into the IRA, which succeeded in avoiding defeat by superior British forces until a truce was negotiated in July 1921. Mulcahy worked diligently to mold his men into a disciplined and professional fighting force that would be perceived as a legitimate army rather than the band of criminals portrayed in contemporary British propaganda. The author shows that Mulcahy was more careful of the lives of Irish civilians and soldiers than many republican memoirs and subsequent historical accounts have suggested. Her analysis of the strategic and tactical considerations governing Mulcahy and his staff complements Charles Townsend's *The British Campaign in Ireland, 1919–1921* (1975) and David Fitzpatrick's *Politics and Irish Life 1913–1921* (1977).

A central theme is Mulcahy's concern for the accountability of the military to the popular will and to democratically constituted government. Mulcahy was typical of many young Irishmen whose political convictions as members of the Irish Republican Brotherhood and later of the Irish Volunteers eventually led to revolutionary military activity. The nature of this process spawned the classic problem of defining the proper relationship between the military establishment and the government officials acknowledged by the people as their legitimate leaders. During the Anglo-Irish War, Mulcahy often reminded his field officers that even guerrilla war had to be conducted within the moral limits espoused by Irish popular will. Later, although personally disappointed with the limited dominion status arrangement of the Anglo-Irish Treaty of 1921, he insisted that it was the army's duty to accept the settlement approved by the Dail representatives until an election demonstrated majority support for the militant republican views that were held by those IRA officers who believed their organization had quasi-political authority. Indeed, Valiulis shows that Mulcahy, torn by the potential shattering of the military cohesion he had built and sensitive to the disillusion felt by those who sacrificed so much in fighting the guerrilla campaign, exerted more energy than leading Dail politicians such as Arthur Griffiths and Eamon de Valera to find a compromise and avoid the drift into civil war during 1922. Yet his efforts proved fruitless and the ensuing civil war, lasting from June 1922 to mid–1923, caused more civilian deaths than the War of Independence, and left Irish party politics deeply scarred until recently.

During this bitter and politically unstable era from January 1922 to 1924, Mulcahy's commitment to the principle of civilian control of the military was severely tested. Electoral results having proved the people's acceptance of the treaty settlement, Mulcahy implemented the policy of the pro-treaty provisional government and built a new and efficient army of 40,000 to field against the republican militants. Unfortunately, as the author demonstrates, his concern to end this conflict as quickly as possible led him to support harsher tactics than were politically wise or even just. For example, the execution of Erskine Childers in November 1922 for possession of a pistol, and two weeks later the execution of four republican prisoners in retaliation for the assassination of the pro-treaty Dail deputy Sean Hales, were errors of judgment that damaged the reputations of Mulcahy and his Cumann na nGaedheal cabinet colleagues for many years.

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Ironically, having demonstrated his commitment to civilian control of the military and thereby enduring the personal anguish of waging war against old comrades like Liam Lynch, Mulcahy eventually resigned his position as minister of defense in 1924 when his Cabinet colleagues, partly in search of a scapegoat for their own political vulnerabilities, subverted his efforts to prevent disgruntled military officers in the Irish Free State Army from interfering in the determination of government policy. Valiulis contends that Mulcahy's selfless patriotism and principled leadership during this controversial era was crucial in helping Ireland, unlike many other newly liberated countries, to avoid rule by a military junta.

In this comprehensive and probing narrative, Valiulis makes a strong case for including Mulcahy along with Michael Collins, Arthur Griffiths, and Eamon de Valera as one of the principal founders of independent Ireland.

CATHERINE B. SHANNON
Westfield State College

MARC MARTIN. *Trois siècles de publicité en France.* (Histoires, Hommes, Entreprises.) Paris: Odile Jacob. 1992. Pp. 430. 280 fr.

Marc Martin's book is the first comprehensive history of French advertising and is indispensable reading for historians interested in advertising, French consumerism, and the mass media. Martin analyzes advertising's slow and uneven development across three centuries, the progressive diversification of its forms in relation to various media, its difficult acclimation within French society, and the reasons for French advertising's "underdevelopment" prior to the mid-twentieth century. His investigation proceeds within specific parameters: the dialogical relationship between the development of advertising and the mass media, and advertising's history as a business. Martin does not focus on the evolution and contents of the advertisements themselves.

Martin's account begins in the 1630s with the nearly simultaneous founding of both the French press and French advertising. Early French advertisements were, in effect, classified ads that appeared in their own separate publications. This situation remained fairly stable until the 1820s, when Parisian daily newspapers began accepting advertisements. Despite the development of early agencies, advertising was mistrusted as an arm of "charlatans" hawking miracle cures. As such, ads appeared in the press only on the last page. Despite important innovations in the late nineteenth and early twentieth centuries, notably the rise of mail-order catalogues and artistically sophisticated posters, the use of advertising by certain "dynamic" sectors such as automobiles and mass-produced goods, and the development of a new professional milieu in the 1920s, France continued to be "structurally confined in its advertising underdevelopment" (p. 406). This was due in part to the predominance of small businesses and regional markets, as well as continued public mistrust of advertising.

A series of transformations during the 1930s finally broke through restraints on advertising's growth, notably the development or expansion of new media (radio and the cinema), press innovations, and changes in attitudes. Martin believes that France entered the "age of advertising" only in the economically prosperous postwar era, when a new breed of industrial managers, influenced by the American model, began to consider advertising as a way to enlarge markets for mass-produced goods. French advertisers modernized their techniques and expanded their repertoires to include market research and motivational studies. The tremendous growth in mass consumption sectors—detergents, housewares, autos, furniture—in turn fueled investments in advertising, and hostile attitudes toward advertising virtually disappeared. The extraordinary growth of postwar advertising anchored the industry as a strong sector of the French economy and an essential economic element of the mass media. Martin also argues that the (re)constitution of the large agencies after the war, especially Havas and Publicis, contributed to French advertising's "take-off." The strength of French agencies allowed them to resist the onslaught of American advertisers, which in turn preserved several of French advertising's national peculiarities.

A broad overview such as this inevitably faces limits. Because Martin tethers his narrative to the development of the mass media and frames his analysis within business history, he mentions, but does not fully analyze, cultural developments associated with the historical development of consumerism. This is a different kind of history than the excellent explorations of the interrelationships between advertising and consumer culture written by Anglo-American historians such as T. J. Jackson Lears, Roland Marchand, or Thomas Richards. Perhaps this neglect of consumer culture is one reason that Martin portrays advertising as essentially conservative prior to the 1930s, whereas scholars who examine advertising in conjunction with consumer culture find it to be a leading edge of modernity in the nineteenth century.

This book is a major contribution and deserves to be translated in order to reach the broadest possible audience. Based on primary, although not archival, sources, it is the best available synthesis of French advertising history.

ELLEN FURLOUGH
Kenyon College

CATHERINE VELAY-VALLANTIN. *L'histoire des contes.* Paris: Fayard. 1992. Pp. 359. 150 fr.

Catherine Velay-Vallantin has written a cultural history of six tales, each of which originated in local oral tradition and then evolved into several different

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stories that circulated throughout literate Europe. Even these later versions continued to change from edition to edition in new historical circumstances, including oral culture itself, long after the invention of printing. As the author states, "The tale, inscribed in the *longue durée*, is not for all that an unchanging object" (p. 39). With such an approach, she corrects the ahistorical perspectives of previous studies by ethnographers like Stith Thompson (1961), structuralists like Vladimir Propp (1928), and psychoanalysts like Bruno Bettelheim (1975). The result is a more complex account of both literate and preliterate cultures and their relationships over time.

Velay-Vallantin's book focuses on Blue Beard, the Girl without Hands, the Venus of Ille, Geneviève of Brabant, the Girl-Soldier, and Saint Marie-Madeleine of Marseilles. In each case, the author traces the historical sources for the most recent version. Geneviève of Brabant, for example, had its latest incarnation in a song still sung in Loire-Atlantique as late as 1985, thanks to family copybooks that recorded local traditions, despite or even because of a very different printed version set down by René de Cérziers in 1665. Cérziers's "pious romance" itself was based on even older, medieval tales recorded by Jean Gobi, Gauthier de Coincy, and Vincent de Beauvais. In the intervening years, from the anonymous *Gesta Romanorum* to Marcel Proust's appreciation in *A la recherche du temps perdu*, this tale appeared in no fewer than a dozen versions, including miracle plays, dramatic poems, *bibliothèque bleue* abridgements, religious processions, *faits divers*, children's marionettes, and café-concert songs. Clearly, both producers and consumers refashioned the original tale about the misfortunes of an innocent woman in ways that were appropriate to their period and place, as Velay-Vallantin explains for each story.

This work is an *Annales*-style *histoire totale* of folk tales. By tracing variations over time from region to region, the author illuminates the complex interactions between oral and literate cultures in precise historical contexts. In this way she adapts literary history and ethnography to the new history of the book as practiced by Roger Chartier and Daniel Fabre, Velay-Vallantin's mentors at the École des Hautes Études en Sciences Sociales: "the tale is not just an expression of the past" but also a reinvention of tradition by tellers, publishers, audiences, as well as writers (p. 11). The author's attention to the interpretive features of texts also recalls the interest of intellectual historians in literary theory, particularly Michel Foucault's concern with discourse, one that is structurally and functionally independent of both producers and consumers, like the pardon tales studied recently by Natalie Zemon Davis (1987). For Velay-Vallantin, each version of a tale becomes a reinterpretation of an earlier text whose meaning is adapted to changing cultural traditions, whatever the intentions of their creators.

The rich detail and the scrupulous handling of the material make this exemplary intellectual history. There is no intrusive jargon, no quest for illusive *Ur*-texts, and no simplistic generalization about national cultures. Rather, it is a well-informed investigation into the ways stories change and why. Thanks to Velay-Vallantin's carefully crafted case studies, folk tales once again deserve the attention of intellectual and cultural historians.

JAMES SMITH ALLEN
Southern Illinois University, Carbondale

JOHN R. COLE. *The Olympian Dreams and Youthful Rebellion of René Descartes*. Urbana: University of Illinois Press. 1992. Pp. xi, 295. $34.95.

After René Descartes's death in 1650, a notebook was found labeled "Olympica" describing three dreams he had had on the night of St. Martin's eve, November 10, 1619, and giving his interpretations of them. The notebook has disappeared, but its alleged content was translated into French in 1691 by Adrien Baillet when composing his biography of Descartes.

Most Cartesian scholars have ignored the text, finding it unphilosophical. Some have questioned the reliability of what survives. Did Baillet accurately render the original material? Can we trust Descartes's account? When was it written down? At the time of the dreaming, or some time afterward? Have we any way of judging this at the present time? Sigmund Freud was asked in 1928 to interpret Descartes's dreams, but he said he could not without knowing the dreamer and his world. Obviously, something important occurred in Descartes's life in November 1619, otherwise why write *Olympica*, and why keep the notebook with him to the end of his life?

John R. Cole claims that recent research on dreams enables us to assess the material. Based on present information about dream remembrance, Cole argues for Descartes's having recorded the dreams shortly after their occurrences and having interpreted them soon afterward. Cole's book offers some striking new possibilities for interpreting the dreams. First, he points out the significance of St. Martin's Day in Descartes's life: he finished his legal training that day in 1616; he entered the Netherlands and met the Dutch scientist, Isaac Beeckman, that day in 1618; and the courts began their legal year on St. Martin's Day with much pomp and ceremony. Second, Cole shows the significance of Descartes's decision to reject taking up a legal career. His father, his brothers and half-brothers, and his in-laws were lawyers, and the status of the family was due to their involvement in the legal establishment. In rejecting a legal career Descartes also rejected his family and their world. Third, Cole describes the intense relationship between Descartes and Beeckman. Beeckman, one of the first to develop mechanistic solutions for physical problems, was more than a decade older than Des-

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cartes. He immediately recognized the young Frenchman as a mathematical prodigy. They worked together excitedly for several weeks, but then they had a nasty falling out. Beeckman was the most important influence in getting Descartes started in his scientific-philosophical career, and Descartes by the time of the dreams had rejected his Dutch mentor.

Using this material about the traumas in Descartes's life, Cole shows how the various elements of the dreams can be interpreted as being Descartes's rebellion against law, family, and mentor, and a declaration that he was going to live as an independent truth seeker. Using a wealth of historical information, Cole gives us what is probably the most complete interpretation available.

Nonetheless, details of interpretation are still subject to question, since we do not know who else Descartes knew during the period, nor what else had occurred in his life after he left the Netherlands. Did he, as some believe, get involved with Rosicrucians? Could this have been on his mind? Comparing Cole's interpretation of dream symbols with what others, including Gregor Sebba (*The Dream of Descartes* [1987]), have argued, one sees that there are other possible associations that might be relevant.

Cole's work nevertheless is the most substantial interpretation that we have. He presents us with a psychologically believable Descartes and justifies the suspicion that the night of November 10, 1619, was the turning point in Descartes's intellectual life. It seems clear that the great dream night changed his outlook and led him to become the father of modern philosophy.

Yet Cole is perhaps too aggressively dismissive of those who have previously worked on Descartes's reported dreams, such as Henri Gouhier and Sebba, and he is too quick to reject possible Rosicrucian elements in Descartes's imagery.

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K. STEVEN VINCENT. *Between Marxism and Anarchism: Benoît Malon and French Reformist Socialism.* Berkeley and Los Angeles: University of California Press. 1992. Pp. xiv, 193. $30.00.

In this brief, well-written, and enlightening volume, K. Steven Vincent argues persuasively that Benoît Malon (1841–93) belongs at the very heart of the French socialist tradition. He did as much as anyone in the nineteenth century to forge a socialist alternative that was neither Marxist nor anarchist, although that alternative owed something to each. Malon's personal journey, beginning (unlike most of his fellow socialist publicists) deep in poverty and manual labor, wound through the Proudhonian wing of the First International, veered toward a moderate collectivism in which he believed that decentralized ownership of property should be in the hands of worker cooperatives, and then led him into non-sectarian organizing work, at which he was quite adept, first among Parisian workers for the International and subsequently for the siege and the Commune. In the Commune, the trait of conciliator and synthesizer already apparent in his ideological shifts served the movement well until the crisis in late April and the creation of the Committee of Public Safety, which he condemned as a "usurpation of the sovereign rights of the people." Malon's stand against Jacobinism and centralized authority remained a constant in his thought. He nevertheless fought valiantly for the Commune and eluded capture, ultimately escaping to Geneva in July 1871.

Nine years of exile in Switzerland and Italy took Malon through various anarchist pathways, including contacts with James Guillaume, Michael Bakunin, and an assortment of Italians, which solidified his opposition to violence and his dedication to debate, conciliation, and experimentalism within the socialist camp. He also became increasingly aware, through his partnership with Andrée Léo (nee Léodile Béra), of women's oppression and the failures of socialism to address it adequately. Malon went well beyond most male socialists in his endorsement of full political and professional equality for women, but Vincent is distressingly vague about the ideas and influence of this intelligent feminist with whom Malon spent nearly a third of his life.

Malon's mature "integral" socialism emerged after his return to France in the heat of the socialist battles of the 1880s. Encounters with Marxism in the stolid writings and ham-handed politicking of Jules Guesde and Paul Lafargue only enhanced Malon's mistrust of what he termed its "social fatality" and his fears of its authoritarian potential. Although ideologically more in tune with Paul Brousse, he and a growing circle of allies were dismayed at the Possibilist leader's political intolerance. Thus, the *Revue socialiste* came into being in January 1885. Its goal was to serve as an open forum of socialist ideas and as the basis of a non-sectarian, independent socialism in France. In that function it was the first great organ of modern French social democracy, tracing the road, for better or worse, leading from Jean Jaurès to Léon Blum to François Mitterand. In Vincent's mind, the change was definitely for the better. Integral socialism was parliamentarian, reformist, *autogestionnaire*, and moralistic. Labor was the true source of republican virtue and workers were the true citizens, patriotic in that they would fight in defense of a social-democratic France. Vincent details the various elements of Malon's utopia clearly and offers a thoughtful defense of the foundation of his thought: the belief that history moved in progressive stages (with intervening periods of disassociation) toward increasing human sociability and what held it together, altruism.

Vincent's effort to rescue Malon has much to recommend it, but there remain difficult questions about

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Malon's heritage. He clearly was not some harbinger of Vichy, but one might argue that his tolerance of an occasional piece of socialist anti-Semitism in the *Revue* betrayed an ongoing mentality that might not see the struggle against the deportation as a top priority of the Left. That Georges Lefranc was Malon's chief defender in the 1930s and 1940s is troubling. Malon was indeed well-advanced on the "woman question," but in the long run he seemed more concerned about family reform than women's full equality. Moreover, one has the feeling that there really was not much substance to Malon's thought. His writings are replete with proposals and recommendations. He had views on all sorts of things. But when one asks how he justified his positions, how they were supported by anthropological, historical, or hermeneutic analysis, we come up empty-handed. Vincent is aware of this lack of substantive argument in Malon's thought but fails to wrestle with its implications. The most important implication may be that social democracy still has too fragile a foundation to stand against the powerful arguments of economic and political liberalism. Certainly the recent retreat of European socialism before "entrepreneurship" has exposed its vulnerability. This should make us think twice, perhaps, before joining in the final requiem for Marxism.

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JAY R. BERKOVITZ. *The Shaping of Jewish Identity in Nineteenth-Century France*. Detroit: Wayne State University Press. 1989. Pp. 308. $34.95.

In this fine work on the intellectual history of French Jewry, Jay R. Berkovitz considers the unique Franco-Jewish identity that developed in the wake of the French Revolution's emancipation of the Jews. Structuring his volume chronologically, Berkovitz begins with a review of the late eighteenth and early nineteenth-century debate over the granting of legal equality to the Jews of France, a debate in which both gentile and Jewish writers accepted the premise that traditional Jewish life carried with it certain corrupting influences and that Jews would have to alter at least some of their social, economic, and religious behaviors in order to function in a modern society. Berkovitz then considers the period that followed the meeting of the Paris Sanhedrin convened by Napoleon, a time during which Jewish citizenship was taken for granted and discussion centered on the degree to which Jews could and should be assimilated into French society.

Ultimately, Berkovitz describes the crystallization of a systematic program of *régénération* in the 1830s. Accepting some of the criticisms leveled at Jews but rejecting the desire of many gentile observers to see their complete assimilation, the Jewish leaders who advocated *régénération* attempted to create a Franco-Jewish identity resting on a "symbiosis of two mutually enriching civilizations" (p. 251). The ideology of *régénération* accepted the need for a socioeconomic transformation of Jewish life, and it also demanded the modernization of education, the elimination of certain religious rituals that were not in accord with French mores, the transformation of rabbinic training, and the unification and centralization of the French Jewish community. In the brief final section of his book, Berkovitz discusses Jewish identity under the Second Republic and Second Empire, when the full impact of emancipation and of the intellectual developments of the postrevolutionary period were finally felt.

In the course of recounting the evolution of a modern Franco-Jewish identity, Berkovitz presents masterful analyses of many of the relevant texts, documents such as the Abbé Grégoire's proemancipation *Essai sur la régénération physique, morale et politique des Juifs*, Orly Terquem's highly reformist *Lettres Tsarphatiques*, and the definitive *ordonnance* of 1844 governing the administration of the French Jewish community. Berkovitz's book also contains fascinating material on the lives of many rabbinic and community leaders, a helpful description of the Jewish educational system of nineteenth-century France, insightful commentary on the rabbinic literature of the period, and useful allusions to the historiography of Franco-Jewish history. The book is strengthened also by its concern with context. Berkovitz does an excellent job of linking Franco-Jewish thought to the Haskalah (the Jewish Enlightenment); he perceptively compares the ideology of *régénération* to the concept of Reform in German Judaism; and he makes frequent references to issues of *mentalité* in nineteenth-century France more generally.

Although on the whole his book is an extremely perceptive and valuable study, some of Berkovitz's broader assertions are not supported as convincingly as one would hope. For example, Berkovitz suggests in his introduction (p. 16) that "a single-minded quest for unity" was perhaps "the most enduring feature of nineteenth-century French Jewish history," but his book does not adequately demonstrate that a quest for unity was any more important than other elements constituting the modern identity of French Jews. Indeed, the book contains ample evidence that many Franco-Jewish leaders rejected the kind of unity sought by the proponents of *régénération* and that the goal of unity within French Jewry remained rather elusive. So too, Berkovitz asserts that the advocates of *régénération* "regarded themselves as part of a movement," but his book shows more that they shared a general ideology than that they acted in concert to foster its implementation.

Until only a few decades ago, works in Franco-Jewish history seldom considered the experience of French Jewry beyond the revolutionary and Napoleonic eras, perhaps believing that a separate Jewish history became meaningless after Jewish emancipation had been achieved. More recent works on the

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Jewish experience in France have demonstrated clearly, however, that French Jews continued to have a history of their own even after they were granted legal equality, and that the story of postemancipation French Jewry is a rich and fascinating one well worth exploring. Berkovitz's book is yet another example of the excellent works being produced by contemporary scholars delving into the modern history of the Jews of France.

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RICHARD VINEN. *The Politics of French Business, 1936–1945*. New York: Cambridge University Press. 1991. Pp. xv, 247. $54.50.

This book challenges the common assumption that French businessmen felt a strong affinity for the Vichy regime. Employers, Richard Vinen argues, had already taken revenge on the Popular Front when labor lost the general strike of November 1938. After the Daladier government suppressed the Communist Party in 1939, businessmen had little more to gain in the social combat of the late 1930s. During the occupation, when Vichy ideologues tried to reorder the economy along corporatist lines, many businessmen were indifferent or hostile. Leaders from heavy industries in particular, located mainly in the northeast, had little stomach for the right-wing visionary schemes of a government based in the south that offered scarce relief from labor shortages and the Nazi boot. Indeed, enthusiasts for corporatism came mainly from the southern zone, especially Lyons, and from small, declining industries. What's more, once the United States entered the war, says Vinen, many businessmen realized Hitler's New Europe was a bad bet, and they scrambled to make discreet overtures to labor and the resistance. If these efforts did them little good at the liberation, Vinen urges us to recognize that the picture of a *patronat* thoroughly wedded to Vichy is largely an artifact of liberation propaganda and historians too ready to echo the antibusiness prejudices of 1944.

Vinen also challenges the notion that Vichy paved the way for postwar economic modernization through the forging of an inchoate consensus between state technocrats and big business. In contrast to Stanley Hoffmann, Robert Paxton, and other historians who see in Vichy a struggle between traditionalists and modernizers (who gradually prevailed), Vinen emphasizes the internal solidarity of the regime. Traditionalists such as Léon Gingembre and modernizers such as Jean Bichelonne, he argues, found enough common ground to create a "fusion" of old and new—a vision of how small and medium-sized companies might innovate without destroying the old social structure of small producers and petty proprietors. If wartime conditions prevented these men from putting their schemes into practice, they did anticipate what Vinen sees as the secret to France's postwar economic success: an alliance of Gaullists and small entrepreneurs who delivered a peculiarly French combination of economic growth and traditional social order in the Fifth Republic.

This book offers an unusual mixture of sound argument and thinly documented speculation. Vinen's careful research in business and government archives allows him to reconstruct how business leaders addressed major issues in labor relations and Vichy corporatism. Vinen marshals his evidence skillfully to show how diversified and conflicted the business community remained during the occupation, showing, for example, how differently various business sectors responded to Nazi efforts to recruit French labor. He is surely right when he says that many businessmen, especially the industrialists in the north, felt they were better off in 1939 with a conservative Third Republic governed by Édouard Daladier and Paul Reynaud than with the new order of Adolf Hitler, Philippe Pétain, and Pierre Laval.

But Vinen deliberately confines his analysis mainly to key spokesmen and major business associations, and hence to the high politics of business-state relations. His occasional forays into the economics and day-to-day realities of business—through several excellent passages on the coal industry—reveal how much more there is to learn by probing behind the associations. Likewise, his startling ideas about the traditionalists and small business roots of France's postwar economic growth call for much more evidence than he offers here. So, too, does his claim that leaders from heavy industry virtually allied with the renascent labor movement after 1942. Despite these shortcomings, however, this book's fresh material and imaginative arguments shed bright new light on a neglected field.

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W. W. MEISSNER. *Ignatius of Loyola: The Psychology of a Saint*. New Haven: Yale University Press. 1992. Pp. xxix, 479. $35.00.

This is a challenging, engaging, but, from a historical perspective, problematic book. W. W. Meissner, a noted psychoanalyst, sets out to explore Ignatius of Loyola's personality. He candidly admits the difficulties involved in probing the unconscious of a subject separated from the analyst by almost four hundred years. He also underscores the limitations of the available sources, although there is plenty of documentation. What Meissner does not question is the validity of applying Freudian psychoanalytic theory to a society where family life was very different from that of late nineteenth-century Vienna. On the contrary, he accepts the universality of the psychoanalytic method, and herein lies both the strengths and the

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weaknesses of this, the latest of many, often equally problematic, attempt at historical psychobiography.

The volume's strengths lie in its readability and Meissner's command of the psychoanalytic literature that constitutes his point of departure. It begins with a lively, although largely derivative, narrative of the future saint's life. What is new and often quite fascinating is the accompanying psychoanalytic commentary on Loyola's inner life. The final third of the book offers a detailed analysis of Loyola's mysticism and concludes with a reconstruction of what is diagnosed as his conflicted but by no means psychotic personality. In Meissner's opinion, Loyola combines "The mentality of a phallic narcissistic hidalgo, who dreamed of heroic exploits and libidinal triumphs" that was in constant struggle with an "internalized, prohibitive maternal system . . . permeated by profound religious and moral values" (p. 336).

On what basis does this diagnosis rest? Not very much, unless one accepts the premise that Loyola's adult personality grew out of "the infantile need for union with a loving mother and father that Iñigo had been deprived of in his early years" (p. 342). The facts supporting this hypothesis are disappointingly few, partly because Loyola's autobiography—one of Meissner's main sources—offers scant details about his childhood years. It is true that Loyola's mother died shortly after his birth, leaving him to be raised by a wet nurse and a series of other women. This was an experience common to countless other sixteenth-century children, only a tiny proportion of whom were mystically or spiritually inclined, but Meissner makes Loyola's quest for his "lost mother" a determining element of his personality, the driving force behind his mystical ascent. Another determining factor in his personality was a deep yearning for the father "lost" early in life. Meissner reveals his presuppositions about the early modern family when he writes that "Beltrán [Loyola's father] . . . was not much of a family man" (p. 334). By this standard, most Spanish children, especially those of the nobility, shared Loyola's longings for a father. Although this may well have been the case, Meissner's narrowly psychoanalytic approach could benefit from a deeper understanding of sixteenth-century history.

In this regard, it is telling that neither Martin Luther nor John Calvin appear in the book's index (interestingly, Calvin studied at the same Parisian college as Loyola); that the Protestant challenge to Catholicism is mentioned only in passing; and that little is said about the spiritual climate of the Spanish church and the resistance of some of its more powerful members to internal reform. Thus, when Meissner discusses Loyola's conversion from soldier to self-proclaimed religious reformer, the devil Loyola contends with is an internal one, a kind of inner ghost impervious to and removed from the momentous religious events of the day. The lens through which Meissner views Loyola's mysticism is equally narrow, focusing on personality conflicts and virtually ignoring the influence of what the Inquisition then regarded as a potentially subversive spiritual movement. Similarly, Meissner side-steps Loyola's military experience and suggests that the Jesuits' hierarchical organization and strict rules on obedience were by-products of Loyola's psychic struggle, especially his efforts to keep his phallic narcissistic urgings under some form of superego control.

In his introduction, Meissner writes that "the approach in this study may be regarded as reductive, it is not intended to be reductionistic" (p. xxvi). This statement implies that his understanding of Loyola is based not on a spiritual or theological reading, but only on those aspects of Loyola's life that can be comprehended in psychoanalytic terms. At this Meissner succeeds, at times quite suggestively. Nevertheless, his approach is reductionistic to the extent that the Loyola he creates, however vital, is a decontextualized persona isolated from the culture and society in which he lived.

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R. A. STRADLING. *The Armada of Flanders: Spanish Maritime Policy and European War, 1568–1668*. (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1992. Pp. xix, 276. $59.95.

R. A. Stradling states in his preface that this is an analysis "of naval policy in the western European theater of the Spanish Monarchy's wars" that focuses on the role of the "Armada of Flanders" and includes the "cognate themes" of privateering as part of "mainstream naval strategy" and the central place of Dunkirk. He admits that "its touch is light (and often derivative)" on economic and technical questions, naval policy in other theaters of war, and issues of international and maritime law, but he partly justifies this on the grounds that there are monographs that cover these aspects (p. ix). The book is divided into three chronological parts (1568–1621, 1621–40, 1640–58) and a thematic section dealing with ships, men, administration, and prize-taking.

The coverage of the book is certainly uneven. Stradling deals with the period up to 1618 in a brief prologue, which is based on secondary material. Yet Stradling makes substantial claims that he does not adequately deal with: for example, that the revolt of the Netherlands would not have continued had Philip II possessed a fleet in the area (p. 3); or that the Portuguese fleet was a factor in Philip II's invasion of Portugal and prevented the development of a "permanent naval apparatus capable of organising the North Sea–Atlantic dimension" (pp. 6–7). Regrettably, the author does not take account of the contrary arguments of J. L. Casado Soto on the North Sea–Atlantic front and the development of Spanish shipbuilding (*Los barcos españoles del siglo xvi y la Gran*

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Armada de 1588 [1988]; a summary of his arguments in English is in M. J. Rodríguez-Salgado and S. Adams, eds., *England, Spain and the Gran Armada, 1585–1604* [1991], 95–132). Despite the recent monographs cited, a historian of Stradling's standing would hardly have failed to add to our scarce knowledge of the "Flanders armada" prior to 1618.

There is little by way of background to put the king's policy on the Flanders fleet into context. Stradling uses much primary material, especially on the period after 1620 (which is covered in great detail), and he assumes knowledge of the monographs cited and of such important issues as the *Almirantazgo* policy. Crucially, Stradling omits a full discussion of privateering, touching only on those aspects raised in his documentation. While he appreciates that the crown's reluctance to accept a pro-privateering policy (implemented in the northern front from 1620) might be linked to "the problem of the Barbary corsairs" (p. 46), and he cites Colbert's opinion of privateers (p. 141), he does not refer to the long debates in Madrid about the legality, morality, or political and commercial impact of corsair and privateering activity, largely, I suspect, because they were rehearsed in the context of Mediterranean policy. This, the limited material on economic issues, and his decision not to study in depth the "private privateers" whom he says accounted for 85 percent of privateering activity in Dunkirk, restrict the book's usefulness as a study of privateering.

Stradling's main contributions are in his discussion of the first half of the seventeenth century. He successfully shows the importance of Dunkirk and of the privateering war, as well as the crucial role of the Armada of Flanders in reopening maritime links between Spain and the Netherlands from the mid–1620s. He is at his best in summarizing the policy debates at court and brings out sharply the importance of maritime strategy to Olivares and the government from 1623 onward. We learn of the tensions between the governments in Madrid (which covered most of the costs) and Brussels for scarce military resources and, just as he intended, Stradling adds solid support to the thesis that Spain remained a major naval power after the defeat of the 1588 Armada.

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J. K. J. Thomson. *A Distinctive Industrialization: Cotton in Barcelona, 1728–1832*. New York: Cambridge University Press. 1992. Pp. xix, 347. $89.95.

It is curious that the pioneering region in Spain's industrialization, Catalonia, has lacked a modern, comprehensive study of its early industrial growth. Pierre Vilar's monumental history—one of the most remarkable achievements of recent Spanish historiography—devoted far greater attention to the agricultural and commercial foundations of Catalan economic expansion during the eighteenth century. Vilar never completed his promised volume on industrialization. The early history of Catalan industry has thus been dominated by a succession of monographic studies focusing largely on individual firms and entrepreneurs. J. K. J. Thomson has stepped in to fill this gap with a systematic survey of the force behind eighteenth-century Catalan industrial production, cotton textiles. The results are impressive.

The manufacture of *indianes*, or printed calicoes, began in Catalonia in the early 1730s. A few decades later, Barcelona housed the largest concentration of cotton printing shops in Europe and constituted the leading Mediterranean exception to the predominance of industrial manufacture in northern Europe. Calico production was divided into two stages. The early period from the industry's founding to the mid–1780s witnessed the successful consolidation of import-substituting manufacture of low-quality cloths. The second phase, which lasted until the establishment of the first fully mechanized factory in 1832, was a period of crisis and underlying structural change. Cotton manufacture began to shift from its heavy concentration in Barcelona toward towns and rural villages outside the city. In a curious reversal of traditional trends toward concentration, lesser units of production (many now devoted to small-scale yet mechanized spinning of yarn) replaced the large factories that dominated the earlier era.

Thomson offers a meticulous and balanced analysis of this century of intense industrial growth. He focuses on four factors in particular: the role of government in fomenting (or obstructing) national industry, commercial capital investment in the industrial sector, the development of a wide range of market possibilities outside Catalonia, and changes in local supply conditions. (Like many economic historians, he focuses heavily on entrepreneurship and management, and says surprisingly little of the labor force.) Regarding public policy, he finds that the government's role was mixed. Indirect policy measures, especially tariffs, aided the nascent industry in intermittent if crucial ways, while the state's direct intervention was often contradictory and unfavorable in its effects on Catalonia. Patterns of capital investment played a leading role in the industry's development. Thomson is particularly effective in showing how traditional forms of local capital mobilization (especially joint partnerships and other forms of pooling resources and managerial responsibility) helped consolidate Barcelona's leadership in industrial production. When assessing market outlets, the author takes care not to overestimate the importance of Catalan sales to the American colonies. In so doing, he reinforces the arguments of Jaume Torras, Josep M. Delgado, Alejandro Sánchez, and other historians who recently have underscored the crucial role played by Catalan penetration of the peninsular

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market prior to the crisis of the colonial system that began in the 1790s. And in regard to supply factors, the author convincingly demonstrates that industrial growth and innovation need not be accompanied by technical leadership. The technological gap between Catalonia and the industrial leaders of northern Europe (especially England) never closed. The importation of skilled foreign workers, however, allowed local shops to produce cloths of sufficient quality to dominate a large (and protected) market area.

The past decade or so has seen a general rethinking of the history of the industrial revolution. In particular, the protoindustrialization debate has drawn attention to the long-neglected importance of manufacturing in rural areas. One of the merits of Thomson's book is to shift the focus back to the urban sphere. In so doing, he offers a detailed and highly stimulating case study of the path to industrialization taken by a region that was at the same time a national leader and an international follower, one of the many paradoxes of modern Catalan, and Spanish, history.

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CLARA EUGENIA NÚÑEZ. *La fuente de la riqueza: Educación y desarrollo económico en la España contemporánea.* Madrid: Alianza Universidad. 1992. Pp. 355.

In the last fifteen years or so, economic historians have transformed the field of contemporary history in Spain by applying the tools of macroeconomic analysis to the study of Spanish economic and social structures. Clara Eugenia Núñez's study of literacy in contemporary Spain, designed to test the theory of human capital as an essential, if not sufficient, element in economic modernization, is a useful contribution to this dynamic field. Although she is aware of the difficulty of defining and measuring such concepts as literacy, education, and modernization, Núñez argues convincingly for a quantitative approach that allows her to analyze the scope and impact of the Spanish transition to literacy with greater detail and methodological sophistication than earlier studies.

In explaining the relatively tardy onset of the literacy transition in Spain, Núñez demonstrates the value of disaggregating national data by region and sex. Her regional figures for the years from 1860 to 1930, when Spain supposedly reached the threshold of "full literacy," reveal yet another expression of the "two Spains": a northern half in which literacy rates were approaching 100 percent by 1930, and a southern half in which fully 50 percent of the population was still unable to read and write by that date. Isolating male and female rates over this same period leads her to some new conclusions about the causes of this regional differential. In the north, she argues, an early demand for education for boys produced a substantial "gender gap" in literacy rates that began to close after 1910, whereas in the south, where investment in literacy promised few returns to the large population of agricultural day laborers, demand (and literacy rates) remained low for both sexes until the 1920s and 1930s, when state action began to increase the number of schools and encourage school attendance. Thus, to the traditional explanation of an inadequate supply of schools and teachers must be added a lagging demand for education among women throughout Spain and among men in the south.

Núñez correctly points out that, on the supply side, the problem was not only a lack of money but also the skewed allocation of resources by the local governments responsible for funding primary and secondary education until the end of the century. Because ruling elites overinvested in higher education at the expense of primary instruction, Spain simultaneously boasted one of the highest ratios of secondary to primary students in Europe and one of the lowest rates of basic literacy. This disproportion was particularly acute in the south, where local notables most effectively controlled the organs of government.

Núñez's efforts to correlate literacy with economic development are hampered by the lack of data on per capita income for the late nineteenth and early twentieth centuries. Nevertheless, using regional economic data for 1900, she shows that, although there was a mutual and reinforcing influence between a minimum threshold of literacy and economic modernization, literacy had a stronger effect on economic growth than the level of economic development had on literacy. She also demonstrates that a large disparity in rates of male and female literacy had a depressing effect on regional income; when the gender gap was narrow, the positive effects of advancing literacy on the economy were more strongly felt. Núñez's disaggregation of national literacy data thus adds another dimension to our understanding of the regional differentials that have been a source of both economic and political tensions in modern Spain.

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KLAUS-JÜRGEN NAGEL. *Arbeiterschaft und nationale Frage in Katalonien zwischen 1898 und 1923.* (Forschungen zu Spanien, number 7.) Fort Lauderdale, Fla.: Breitenbach. 1991. Pp. 798.

This book by Klaus-Jürgen Nagel deals accurately and exhaustively with the relationship of the new industrial working class to Catalan society in the quarter-century from the loss of the Spanish American War to the establishment of the dictatorship of General Miguel Primo de Rivera. This period witnessed the brief career of *Solidaritat Catalana*, the rise of the *Lliga Regionalista* (representing the upper middle classes), the *Confederación Nacional de Trabajo* (or

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CNT, which represented both urban and rural workers), the *Mancomunitat* as a forerunnner of political autonomy, numerous small but active federalist and republican parties, the populist leadership of Alejandro Lerroux García, and the organization of *sindicatos libres* to combat the dominant influence of the CNT. These decades also included the "Tragic Week" with its anticlerical and antimilitary riots, and the profiteering, espionage, labor, and police violence in "neutral" Barcelona during World War I and its immediate aftermath.

Nagel not only analyzes the specific employer and working-class organizations but also deals extensively with cultural matters, such as the use of the Catalan and Castilian languages in the political, social, and intellectual life of both the middle and working classes; social relations between Catalan and "immigrant" workers within the barrios; the differences between Barcelona and the outlying suburbs; and the importance of primary schools, athletic clubs, choruses, and folk dancing. In the context of his highly specific discussions he evaluates with exemplary objectivity existing studies and the various historiographical currents within the Catalan academic community.

Generally speaking, his interpretation emphasizes the complexity and the fluidity of both ideas and group loyalties. He clearly believes that too much weight has been given both to anarchism and to Catalan nationalism. With regard to the first, he points out that the actual number of anarchists in the CNT and in the various republican and federalist parties was always small. He characterizes the CNT as more "evolutionary and syndicalist" (p. 625) than anarchist in its concern with bread-and-butter issues. Although the CNT indeed opposed the police violence and the military conscription of the state, it also worked to obtain implementation of the existing social legislation and theoretical voting rights. Nagel compares its role in the "modernization" of Catalonia to that of the *Lliga Regionalista*. The CNT for the workers, and the *Lliga* for the bourgeoisie, overcame the intense localism of the previously existing organizations.

With regard to nationalism, Nagel separates the practical aspects of a bilingual society from the ideologies that often mean more to the intellectual elites than to the general population. As of 1898, most of the workers were Catalans, and Catalan was their native language. But the large influx of Aragonese, Murcian, and Andalusian workers in the following two decades meant that at work benches, in stores, at union meetings, in election campaigns, and at picnics, it was necessary to speak both Spanish and Catalan. For some, the greater or lesser public use of either language had ideological importance. But for most people, provided they were not humiliated for their accents or accused of speaking a "dialect," the constant use of both languages was (and today still is) a matter of common sense.

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KURT JOHANNESSON. *The Renaissance of the Goths in Sixteenth-Century Sweden: Johannes and Olaus Magnus as Politicians and Historians*. Translated and edited by JAMES LARSON. Berkeley and Los Angeles: University of California Press. 1991. Pp. xxi, 282. $35.00.

In an elegant study of the writings of two exiled Swedish Catholic clerics in sixteenth-century Rome, Kurt Johannesson has rescued from obscurity one of the most important threats to the reign of Gustav Vasa, the founder of the modern Swedish monarchy. Published originally in Swedish as *Gotisk renässans* (1982), this edited translation by James Larson is a welcome addition to scholarship in English on early modern Sweden, as well as to scholarship on sixteenth-century rhetorics.

The subject of Johannesson's study is the intellectual production of Johannes Magnus (1488–1544) and his younger brother Olaus Magnus (1490–1557), as well as its political program, rhetorical framework, and place in the politics of sixteenth-century Sweden, the Counter Reformation, and the historiographical construction of the modern Swedish state. Of this intellectual production, that for which Johannes Magnus is best known is his *Historia de omnibus Gothuorum Sveonumque regibus*, while his brother is renowned both for his *Carta marina* and *Historia de gentibus septentrionalibus*. The value of these works as historical scholarship has been dismissed by Swedish historians for four centuries, yet Johannes Magnus's presentation of Sweden as the mother of nations and the home of the Goths was quickly mobilized in support of Sweden's great power ambitions, and Olaus Magnus's cartography and work devoted to the natural history of the North have traditionally won praise and provoked lasting interest.

Johannesson breaks important new ground by analyzing these works not as historical scholarship but rather as political briefs in the struggle of the Magnus brothers against the Swedish Reformation and in favor of restoring the old faith and its leading clerical proponents. Unlike earlier observers, Johannesson never forgets that Johannes Magnus was Sweden's last Catholic archbishop and was recognized as such by Rome until his death. This is an important departure from earlier scholarship, which was strongly influenced by Gustav Vasa's triumph as the founder of the modern Swedish state and swept up in a celebration of Vasa's own historian, Olaus Petri, as the father of post-medieval Swedish national historical writing. For the first time, then, we have in Johannesson's book a serious analysis of the writings of the Magnus brothers as a continuous and intri-

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cately constructed plea to the Swedish nobility and clerics—and at times to the Reformation monarch himself—to return to the fold of Holy Mother the Church and to accept their spiritual leadership.

That which was rejected in the works of Johannes Magnus and his younger brother as poor history becomes comprehensible when viewed as contemporary politics. The hundred Swedish kings dating in succession from the time of Noah who populate Johannes Magnus's *Historia de omnibus Gothuorum* can be seen and understood in this light as a gallery of rogues and heroes who serve purposes of moral suasion and propaganda rather than of history. The mythical kings Gostagus (p. 131) and Ulphilas (p. 136) thus serve as scoundrels and tyrants with traits clearly calling to the reader's mind the aspects of Gustav Vasa's character and reign against which the brothers Magnus railed.

In its criticism of Gustav Vasa and his alleged tyranny, the *Historia de omnibus Gothuorum* is a work in the genre of the *Fürstenspiegel*. But to focus solely on the anti-Vasa and pro–Counter Reformation theme of this intellectual production would be to oversimplify, for the Magnus brothers also used their work to rally church support for their cause by playing on contemporary Latin fascination with the Goths and by defending Swedish interests against Danish claims. By elaborating the argument presented at the Council of Basel in 1434 by Swedish archbishop-to-be Nicholaus Ragvaldi that the Swedes were descendants of the Goths of antiquity, Johannes Magnus tapped into and capitalized on the very positive "new picture of the Goths [which], like the Renaissance, was born in Italy" (p. 85). Thus it was that the Swedish archbishop-in-exile mobilized the cultural imagination of his contemporaries to derive political and clerical support for his own and his brother's concerns over the fate of Catholicism on the edges of Northern Europe.

This is an inspiring piece of scholarship that demonstrates the value to historians of understanding the rhetorical devices and practices of the age they are studying. As in the present case, this kind of knowledge is indispensable for penetrating the intellectual, political, and cultural references and inferences that define the framework within which our subjects acted on, and reacted to, the world around them.

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TOMAS SÖDERBLOM. *Horan och batongen: Prostitution och repression in folkhemmet* [The Whore and the Baton: Prostitution and Repression in the People's Home]. Summary in English. Stockholm: Gidlunds. 1992. Pp. 243.

The somewhat dubious title of Tomas Söderblom's thesis hints at the fact that Swedish police, dealing with women accused of prostitution in the 1920s and 1930s, were armed with batons. The thesis and its author have attracted a great deal of attention, not only because the subject is a popular one but also because of the author's conclusion that women who made a living from prostitution did so by choice. Söderblom dismisses both the old assumption, found in police records, that women became prostitutes because of lechery and the more modern image of the prostitute as a victim of society. Instead he argues that prostitution should be seen "as a means of subsistence during one period of the women's life. In retrospect, the choice may be deemed as good or bad, rational or irrational, criminally or socially oriented, yet this does not alter the fundamental conclusion that the woman acted consciously and controlled her own life, and that she herself chose to view the matter in this way" (p. 222).

Söderblom's aim is to write a history "from below." His point of departure is a trunk filled with a young woman's property: an old and dry lipstick, clothes from the 1920s, letters, fragments of poems, garters. The trunk was found in the attic of the Museum of Landskrona, the small town in southern Sweden where the Citadel was situated. The Citadel was an old castle, surrounded by a double moat, and was from 1918 to 1940 a state workhouse for women. The unknown owner of the trunk, in the book fictitiously called Maria, was imprisoned at the Citadel as a vagrant. It was a well-known fact at the time that a female vagrant was a prostitute. Prostitution was not illegal per se, but when the regulation of prostitution ceased in 1918 the old Vagrancy Act of 1885 was used by the police to control women on the streets. Three of four vagrants were men, one of several interesting gender differences that the author chooses to ignore.

Owing to the arduous work of the "Spinning House King," as the director of the workhouse was called, the life of the workhouse and its inmates was documented in detail. The director himself (at his own expense) photographed the prisoners—in the nude. Conversations among the prisoners were listened to and recorded, and letters were opened, copied, and sometimes confiscated. Everything was neatly filed, together with documents from all the other authorities the women met on their way to the workhouse. Inspired by Michel Foucault, Söderblom's aim is to describe the Citadel as a "total institution." This proves fruitful and provides insight into the horrors of the controlled life of the imprisoned women.

But when it comes to his analysis of this exciting source material, Söderblom's indifference to the issue of gender becomes problematic. Because his starting point is the notion that human beings are responsible for their own deeds and lives, there is no reason for him to make an exception for women who have "chosen" prostitution, women who "acted consciously and controlled" their own lives (p. 222). Yet at the same time he writes that the women were subordi-

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nated to men "in virtually all respects" (p. 221). The fact that the author finds no inconsistency between the ideas of women's subordination and women's "freedom" to choose is remarkable. Even more remarkable is his ignorance of modern research on sexuality, prostitution, gender, and power. The author's fondness of Foucault has not led him to Foucault's work on sexuality. Also missing is reference to literature with a more analytical and problem-oriented perspective on prostitution, for example those works by Judith R. Walkowitz, Linda Gordon, Ruth Rosen, Gayle Rubin, and Alain Corbin, to mention only a few. (Söderblom actually uses the concepts of one Swedish feminist historical article on prostitution, but he neglects to mention both title and author.) My conclusion should be read as a plea for further research from a feminist perspective. This book is a highly interesting empirical investigation suffering from severe analytical shortcomings.

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TIMOTHY BAHTI. *Allegories of History: Literary Historiography after Hegel.* Baltimore: Johns Hopkins University Press. 1992. Pp. x, 336. $35.00.

This is a dense and difficult book, one "not for everyone" as Nietzsche might have put it. Timothy Bahti undertakes to study selectively certain aspects of literary historiography in Germany in the nineteenth and twentieth centuries, particularly as they developed in relation to Hegel and German Idealist philosophy. More to the point, Bahti is concerned with the question of how, within the context of German literary and philosophical thought, history came to be understood as allegory. Over the past 200 years, a number of German thinkers have reflected on the peculiar relation between two separate meanings of history, that is, history as story or "representation," and history as the actual "happenings" of former times. The story, which purports to be an account of the happenings, can never completely embrace the whole of what occurred in the past, since the totality of historical events far exceeds the capacity of any story to shape or explain those events. Moreover, written history is not only representation but also an agglomeration of literary signs and markings that convey meaning. What then, Bahti asks, is the relationship between these signs and historical reality? If written history is only about something that is nonidentical with it, can there ever be anything like historical truth, or are historians fated to achieve at best only "allegorical truth"? These concerns lead Bahti to explore yet other relationships within the field of literary historiography, including the relationship between facts and concepts, appearance and reality, exterior events and interior significance, the actual and the interpreted, and, again, the literal and the allegorical. Bahti, of course, does not claim originality in exploring these issues; in fact, part of what he attempts to demonstrate is that virtually every major German philosopher or literary historian since Immanuel Kant and Friedrich Schiller gave considerable attention to precisely these issues.

To investigate these themes, Bahti divides his book into two roughly equal parts. In the first, which is meant to be introductory but contains much interesting material in its own right, Bahti deals with the various "discourses" of history as they developed within a German academic context between the fifteenth and the nineteenth centuries. Here he gives an account of how history gradually freed itself from its subservience to rhetoric and jurisprudence, and eventually became a fully autonomous (although philosophically grounded and legitimized) *Wissenschaft.* In the second part of the book Bahti investigates the theme of history-as-allegory as it was dealt with by a number of German thinkers who were in many respects the heirs of these earlier philosophical and historical discussions, even if they did not recognize themselves as such. At this point Bahti focuses his attention on only a few individuals, and then offers extremely close readings of particular works or parts of works that bear on his theme. Among the individuals and works singled out for treatment are Friedrich Nietzsche's *The Birth of Tragedy,* Georg Lukacs's *The Theory of the Novel,* Erich Auerbach's *Mimesis,* and Walter Benjamin's *The Origin of German Tragic Drama,* "Theses on the Concept of History," and "The Storyteller."

Bahti's textual readings of these mainly twentieth-century writers are often highly illuminating. So are his similarly careful readings of earlier thinkers dealt with in the first half of the book (Schiller, Kant, Fichte, Schelling, and especially Hegel). Nevertheless, because Bahti assumes so much on the part of his readers in terms of philosophical background and general knowledge of German intellectual history, not everyone will be willing or able to follow him from beginning to end. Those familiar with and drawn to the work of Paul de Man—particularly his *Allegories of Reading: Figural Language in Rousseau, Nietzsche, Rilke, and Proust* (1989)—will find much of value in Bahti's book. As a devoted student of de Man, Bahti has extended his mentor's methodological approach to literature into the field of historiography. In fact, his book deploys the very same deconstructive reading strategies that have by now become identified as de Manian.

If this approach to history is what one is looking for, Bahti's study will seem valuable and illuminating; if not, it is not likely to be compelling enough in other respects to hold one's attention through 300 pages of detailed textual commentary.

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MANFRED RUDERSDORF. *Ludwig IV: Landgraf von Hessen-Marburg 1537–1604; Landesteilung und Luthertum in Hessen.* (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung Universalgeschichte, number 144.) Mainz: Philipp von Zabern. 1991. Pp. viii, 321. DM 78.

In 1561–62, Philip of Hesse (d. 1567) provided that his inheritance would be ruled by four landgraves, his heirs by Christina of Saxony. The testament assigned Hessen-Kassel to the first son, Wilhelm, Hessen-Marburg to Ludwig, and the counties of Katzenelnbogen to the two younger sons, but it provided common institutions to preserve a single Hessian entity; for example, common diet, high court, funds, university, and church. Manfred Rudersdorf has written the first modern biography of the second son, Ludwig IV (1537–1604). This is an important book because it sheds considerable light on both Philip's decision of 1561–62 and, after 1576, the failure of Philip's common institutional structures to preserve Hesse. Moreover, using Swabian documents, Rudersdorf reveals much about Duke Christoph of Württemberg's relationship to these events.

Rudersdorf argues that a tense Hessian court atmosphere after 1557 led to the 1561–62 division. Moreover, the so-called "Donation" (by which the sons by the second, bigamous wife, Margarethe von der Saale, were provided small pieces of land and honorific titles) was integrally related with the division of the landgraviate. The family struggle between the heirs threatened the authority of the dynasty and could have spread to the people. Wilhelm and Ludwig also pressured Philip. Acting behind the old landgrave's back, they laid the groundwork for Ludwig's move to Stuttgart for princely training under Duke Christoph. While Philip worked on his inheritance problem, they kept up a solid front in opposition to any great concessions to Margarethe's sons. The brothers got Christoph to act as a guarantor of the testament. Ludwig's agreement to the Testament of 1562, including the Donation, insured its enactment. Much of Ludwig's later policies are attributed to his Württemberg experience (1561–65).

Rising confessionalism prevented the maintenance of common Hessian institutions. After 1576, in defiance of Wilhelm, Ludwig, as a "confessional prince" (p. 258), implemented a second reformation in Hessen-Marburg. A new catechism, the Marburg Catechism of 1578, was adopted, and Ludwig supported the work of a confrontational, anti-Reformed theologian at Marburg. Rudersdorf shows that Ludwig's policies were connected with those of other Lutheran states at the time. A "renewed" Lutheranism culminated in the Formula and Book of Concord (1577 and 1580) and a distinct political stance; namely, these states wished to promote the status quo and remain true to the Habsburg emperor. Meanwhile, Wilhelm and the other Reformed princes challenged imperial authority and demanded toleration and equality for the Reformed everywhere.

Rudersdorf's emphasis on the roles of the landgraves Ludwig and Wilhelm, as well as Duke Christoph, raises problems. Philip is assigned a largely passive role. Is there, however, any hard evidence that the machinations of these younger men caused Philip to decide as he did in 1562? The presentation of the old Philip's religious and ecclesiastical views is more troublesome. Is it true that Philip placed political concerns above religious ones (p. 37)? Does the phrase "confessional-moderate indifference" (p. 207) characterize Philip's views? Was Philip responsible for the split because he left a "relatively open and broadly composed" (p. 208) church, hence, a "pre-confessional" (p. 211) church or an "'unsettled' settlement"? As Rudersdorf notes, Philip's church conformed to the Augsburg Confession and the Wittenberg Concord. Not all scholars will agree that these documents were un-Lutheran. Nevertheless, in explaining Hessian developments, Rudersdorf has clearly illuminated the role of the new Lutheranism, the Second Reformation, and the demise of hopes for a common Protestant agreement.

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KARL HÄRTER. *Reichstag und Revolution 1789–1806: Die Auseinandersetzung des Immerwährenden Reichstags zu Regensburg mit den Auswirkungen der Französischen Revolution auf das Alte Reich.* (Schriftenreihe der Historischen Kommission bei der Bayerischen Akademie der Wissenschaften, number 46.) Göttingen: Vandenhoeck und Ruprecht. 1992. Pp. 715. DM 120.

The recent wave of works commemorating the two-hundredth anniversary of the outbreak of the French Revolution neglected the effects of the revolution on traditional institutions in countries other than France. Karl Härter's book will help fill this gap. Härter follows the paradigm developed by Karl Otmar Freiherr von Aretin in his two-volume *Heiliges Römisches Reich, 1776–1806: Reichsverfassung und Staatssouveränität* (1967). Härter's volume deals with the deliberations and activities of the Imperial Diet from the outbreak of the French Revolution until the dissolution of the Holy Roman Empire in 1806.

The Perpetual Imperial Diet sat continuously in Regensburg since 1663, in the aftermath of the Peace of Westphalia, with the original purpose of clearing up matters not resolved in those treaties. Because of the permanence of the Diet, the estates of the empire stationed their resident representatives there: these normally received instructions from their principals, although on occasion they acted on their own. Hence the Diet really became a congress of ambassadors. Others have written studies of the Diet from the viewpoint of individual estates, but none has at-

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tempted the momentous task of presenting a comprehensive panorama. Härter, however, after diligently searching through ten archives, gives us a wider, more balanced view.

Härter opens his book with a useful overview of the functions and procedures of the Imperial Diet. He admits that the proceedings were slow, cumbersome, and to little effect, attributing these weaknesses to problems of communication and the laggard imperial policy of Joseph II. These early intimations already alert us to the role of the Diet in the conflict with France. It had to cope with the direct and indirect effects of the French Revolution on the constitution and coherence of the empire, both being severely challenged by events. Its task was to preserve and stabilize the existing order while undertaking within the traditional constitutional framework the basic reforms needed to save the empire, perhaps by carrying through a "revolution from above." But in the early years there was also a fear, real or presumed, of a German "revolution from below" on the French model. Hence the need to control revolutionary propaganda, agitators, and a German press sympathetic to events in France. Finally, it had to face the consequences of unsuccessful wars against France, which brought the loss of the left bank of the Rhine and with it the problem of compensating the secular princes adversely affected by the cession. Härter's account is a compromise between a critical analysis and a chronological description of the deliberations and backstairs negotiations going on at Regensburg.

There is no doubt that Härter's thorough and exhaustive treatment will remain the standard work for years. The question remains whether the Imperial Diet played as vital and useful a part as Härter seeks to give it. He admits that, in the face of the divergent interests of Prussia and Austria and the greed of middle-sized German states striving for full sovereign status, the Diet could not carry out its function of bringing about consensus and compromise in the empire. But on reading through the deliberations, even before the factors that destroyed the empire came into play, one cannot help feeling that the Imperial Diet was not much more than a talking shop, although useful for ventilating grievances, expressing opinions, and maintaining contacts among opponents. Perhaps the Holy Roman empire was after all not a political but a juridical entity, and its most effective organs were the imperial tribunals.

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FREDERICK C. BEISER. *Enlightenment, Revolution, and Romanticism: The Genesis of Modern German Political Thought, 1790–1800*. Cambridge: Harvard University Press. 1992. Pp. xiii, 434. $45.00.

Madame de Staël, in *De l'Allemagne* (1810), described German intellectuals of the late eighteenth century as bold in thought but obedient and timid in the face of the authoritarian state. According to Frederick C. Beiser, her view has influenced all subsequent appraisal of the thought of this period and led to the "myth" of the apolitical German. He proposes to set aside this myth by showing that German thinkers of the 1790s were essentially political in motive and objective.

The book has a straightforward analytical framework. Beiser discerns three "patterns of thought" in the 1790s: liberalism, romanticism, and conservatism. He acknowledges quite properly that these terms should not be understood to imply political organization or activity in the modern sense. Over a dozen thinkers are thus classified and their views described. Among them, Immanuel Kant and Friedrich Schiller are treated as liberals, Johann Gottfried Herder and Friedrich Schlegel as romantics, and Justus Möser and Friedrich Gentz as conservatives.

Kant, in Beiser's interpretation, was by no means simply a philosopher concerned mainly with abstruse questions. Indeed, Kant discovered the "primacy of the political." His Copernican revolution was not only epistemological but also moral, for he made the moral world depend on the laws of the will and, thus, ethics became "anthropocentric." Such a view was "radical in the extreme," for now society and state must conform to the individual and not the other way around. Freedom must then prevail over paternalism, however benign. Schiller as well ought not to be seen as flying from the real world to the world of beauty. His *Aesthetische Briefe* (1795), Beiser argues, should be understood not as an appeal for *Bildung* in itself but as a means of educating the desires and feelings of the people, that they might then be ready for a republican order.

The Romantics, asserts Beiser, although often regarded only as a literary movement, were important political thinkers and perhaps the foremost critics of the society of their time. They derived their most salient political views from Herder, who was among the most radical thinkers and not at all part of the "neutral Weimar culture," as has often been maintained. His historicism gave him the basis of his *Zeitkritik* against the artificiality of the Enlightenment but also against the despotic paternalism of the German principalities; hence his rejection of "order and obedience" as principles of state.

Chief among the early precursors of conservatism was Möser, who saw in his beloved *Kleinstaat* an organism that nature and the wisdom of ancestors had produced. His concern, which was primarily political, was to show that the proper constitution is given in the course of time and cannot be contrived. Thus, Beiser leaves us with little doubt that the philosophers, historians, and literary figures of this period were not dwellers in the clouds but were very much engaged with the question of what civil society might best serve humanity.

I have only two questions to raise about this admi-

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rable book. The first is whether the ideological attributions are entirely meaningful. The Kantian imperative of duty lent itself more to subsequent right-wing movements than to German liberalism. Herder and the Romantics may have been "radical" in the sense that they repudiated the Enlightened state, the French as well as the Prussian, and their ideas subsequently were to be set against Stein and Bismarck. Yet political romanticism became a deeply traditional movement, influential through William II and beyond. The second question is whether the discrepancy between Beiser's account and the views of those who treat the Germans as apolitical is as large as he suggests. We may apply the term apolitical to those who, whatever their ideas, were not activists. In this sense the Germans were not as conspicuously political as Beiser indicates. We may find in Germany Rousseaus and Montesquieus, but where are the Marats and Robespierres?

Despite these criticisms, this is an excellent book, erudite, lucid, and interesting. Both the specialist and general reader will find pleasure in reading it.

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KARL H. WEGERT. *German Radicals Confront the Common People: Revolutionary Politics and Popular Politics 1789–1849.* (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Abteilung Universalgeschichte, number 135.) Mainz: Philipp von Zabern. 1992. Pp. x, 370. DM 88.

Karl H. Wegert's study of the relationship between left-wing activists and the common people in southwestern Germany (except for a chapter that veers north to Hanover) from 1789 to 1849 is the latest work on a topic of increasing scholarly interest, the history of early German radicalism. Unlike most writers, who have favorably approached this topic looking for the roots of a democratic countertradition in German politics, Wegert has a far more negative view of late eighteenth and early nineteenth-century leftists. He sees them as impoverished, marginal intellectuals, spouting abstract, utopian doctrines, vainly trying to gain the support of craftsmen, small merchants, and property-owning peasants who were unsophisticated and parochial, but practical, conservative, and orderly. All that came of the radicals' efforts was "recurring misfortunes" (p. 322), the disruption of the reforming activity of wise statesmen, and the persecution of simple-minded common people innocently caught up in the leftists' machinations.

Wegert's thesis is provocative, and he supports it with a mass of archival evidence from judicial records. I have gone through some of the same material on the Palatine high treason investigation of 1849–51 in my own work, however, and it seems to me that Wegert uses these sources in a highly tendentious way: uncritically accepting evidence supporting his interpretations and not using other evidence refuting them; and overlooking other sources produced during the revolutionary events that cast in a different light the ex post facto statements made in judicial interrogations.

Additional problems arise in Wegert's interpretation of his sources. He frequently offers diametrically opposing judgments, so long as they support his argument. Most ordinary people who joined the Mainz Jacobin Club, Wegert asserts, did so out of fears for their future, not because they were radicals; yet rural Rhinelanders' celebration of their annexation by revolutionary France was no sign of radicalism, because to the common people "the future was an irrelevancy" (pp. 27–29, 38–39). Wegert rejects state officials' opinions of the stupidity of common people when he thinks that such judgments overlook their rejection of radicals' ideas, but when officials describe an artisan who was an active supporter of radicalism as too stupid to know what he was doing Wegert accepts that judgment without reservation (pp. 38, 269).

The weakness of the work's main thesis is particularly apparent in the discussion of the 1848–49 revolution, when southwestern Germany was dominated by radicals who formed as many as a thousand political clubs, elected a slate of leftist deputies to the Frankfurt National Assembly, and ultimately seized power in the region, creating revolutionary regimes that had to be destroyed by the Prussian army. All this strongly suggests a good degree of popular support for left-wing political movements, to which Wegert's main response is to make nasty remarks, describing leftists variously as "*enragés* and bellicose crackpots" (p. 256); a "town fool . . . utterly pathetic figure" (p. 249); or a "sinister figure, gaunt and swarthy, black-haired and black-bearded" (p. 242). Participants in democratic mass meetings, he maintains, had "time on their hands and mischief in their hearts" (p. 301).

Unfortunately, incautious and uncritical use of judicial records as sources, questionable and contradictory interpretation of these sources, and name-calling to cover up deficiencies in the argument pervade this book, occurring far more frequently than can be discussed in a brief review. Taken together, they suggest that Wegert has simply imposed his own conservative political views on the empirical evidence he has gathered, repeatedly crossing the line separating an objective work of scholarship from a political tract.

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KARIN LUYS. *Die Anfänge der deutschen Nationalbewegung von 1815 bis 1819.* Münster: Nodus. 1992. Pp. 317.

Karin Luys examines the "elite nationalism" of the Restoration Youth movement, the *Burschenschaft*, and

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several of its mentors, principally in the university towns of Giessen and Jena. The author has made extensive use of the detailed reports prepared by the officers of the Mainz Investigative Commission, established by the federal authorities in 1819 to ascertain the nature and extent of political machinations within the German Confederation. This is a source that has been mined repeatedly by others and is therefore not likely to yield much that is not already known. The reports might still be exploited using an innovative methodology, of a kind not found in the book under review.

Luys presents some interesting, although not very significant, material on that cohort of men conditioned and then mobilized by the prolonged confrontation with France: the semi-political societies that they founded (Hofmann Society, German Reading Society, Darmstadt and Giessen "Blacks," Jena *Burschenschaft*), the links forged among and between these various groups, ideological continuities, and dominant personalities (the Follen brothers, Jakob Fries, Karl Hofmann, Heinrich Luden, the Snell brothers, the Welcker brothers). It is difficult to justify the word "movement" in connection with the restrained nationalist agitating of the post–1815 years. The number of individuals involved was always small, fewer than 100 within any one group. None of the groups managed to evoke a sympathetic response among the masses. Those attempts to extend feelers to a wider constituency (the "petition campaign" of 1817 in support of constitutional change, radical involvement in the Odenwald peasant disturbances) were unqualified failures. "National movement" did not really involve much more than the activities of peripatetic radicals.

The one enduring contribution of Restoration nationalists was their constitutional drafts for a more tightly structured German state. To her credit, Luys has gone further than anyone else to collect, analyze, and compare these obscure manifestos. It is clear that the politically conscious men of 1815–19 could not arrive at any kind of consensus, nor were they capable of projecting original, coherent visions of a new Germany. The Hofmann Society yearned for a liberal nation-state under Prussian hegemony. The Giessen "Blacks" envisaged a German democratic republic resting on ill-defined ethical, folkish, and Christian principles. The Jena program came out squarely on the side of constitutional liberalism. In fairness to the exuberant men of the Restoration, the kinds of problems addressed in these works would bedevil more mature men of a later generation, whose solutions would prove to be no more satisfactory.

The book does not do justice to the English-language literature that is available for this topic. Generational conflict, petition campaigns, cohort analysis, and Restoration ideologies are themes examined in books and articles with which Luys is apparently not familiar. In sum, we have here a narrowly conceived work that has merit but which will not generate much interest.

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HERBERT REITER. *Politisches Asyl im 19. Jahrhundert: Die deutschen politischen Flüchtlinge des Vormärz und der Revolution von 1848/49 in Europa und den USA.* (Historische Forschungen, number 47.) Berlin: Duncker und Humbolt. 1992. Pp. 391. DM 68.

In the nineteenth century, as today, the question of political asylum was a ticklish issue that inevitably entailed the entire political philosophy of a regime. The obligation of states to hand over common criminals for justice had been tempered by a refusal to return those subject to prosecution for their political beliefs, particularly when those beliefs coincided with the philosophy of the harboring state. The Jacobins, for instance, had proclaimed revolutionary France the homeland of all republicans, actively soliciting those guilty of treason to monarchs.

According to Herbert Reiter, in the aftermath of the Napoleonic wars the states of the Holy Alliance made a special effort to pursue those plotting against monarchial legitimacy, because they were seen as a threat to all. The chief exceptions to this European solidarity were the new state of Belgium, which had arisen from a popular revolt against the House of Orange in 1830, as well as "republican" Switzerland and Great Britain, whose definition of treason was differently constructed from the *crimen laesae majestatis* of continental Roman law. The United States, separated from Europe by the broad Atlantic, was passively benign to European revolutionaries and promoted immigration. Belgium, while refusing to hand over most political refugees, tended to try to funnel them to other lands, as did Switzerland in most cases. The result was that Britain and the United States received political immigrants not because they had a policy of political asylum, but rather because they had no policy on the matter at all.

The position of the United States, as the sole large republic in the Western world, was the object of a great deal of ill-informed speculation by European radicals. Although most activists chose Britain as their first haven because of its closeness to the continent and the indifference of British authorities, America received those resigned to the failure of the revolts in their homelands. Efforts to enlist even an expansionist American administration in the service of world revolution proved illusory. The effort by Gottfried Kinkel to raise a German Republican Loan was a rare attempt to enlist German-Americans directly in the service of revolution, and it failed to do much more than upset European officials. Renegade American petty diplomats, such as Consul G. H. Goundie in Basel, were acting against the expressed policies of their government. (Letters by Goundie to Friedrich

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Hecker in the Hecker Papers at the Western Historical Manuscripts Collection at the University of Missouri, St. Louis, show that this official was even more deeply involved in radical activity in 1848–49 than Reiter knows.) Ultimately the Americans accepted the immigration of radicals as a small part of a massive economically spurred migration from Europe. This had the effect of making the United States the moral equivalent of a penal colony for continental Europe.

Reiter's book is a well-written, thoroughly researched study that will provide readers with the best orientation currently available on the broader context of the Forty-Eighters. For that very reason, an index of names is deeply missed. The exposition here of intelligence reports on radical activities passed to European states, particularly to the Prussian government, is a revelation certain to be followed up now that Potsdam can be reached by streetcar from West Berlin.

Steven Rowan
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Paul Lawrence Rose. *Wagner: Race and Revolution.* New Haven: Yale University Press. 1992. Pp. x, 246. $25.00.

The easy compartmentalization of Richard Wagner into odious personality, derivative ranter, and glorious composer is challenged by Paul Lawrence Rose in a relentless, eloquent, and lopsided polemic. The argument corrects the neat distinction between youthful revolutionary and postrevolutionary racist favored in the recent past. Instead of rupture, Rose stresses a continuity from the Young German "revolutionist," obsessed with ethnicity, to the Bismarckian *Dichterfürst.* The composer's racism and revolutionism is part of a package deal: "The idea of revolution was an epiphany that shook his marriage, altered his whole conception of his art and opera, and set him on a permanent revolutionary path, which, despite its ideological and political windings between liberalism, socialism and monarchism, was always directed by an abiding antisemitism" (pp. 49–50).

Rose demonstrates how the religious politics of the *Vormärz* prepared the full-blown racism of the late nineteenth century, but he muddies his own argument when he speaks of Wagner's "revolutionary ideas of 1848–50" (p. 61) as if the defeat of the revolution did not matter at all. The *Artwork of the Future* (1849) is called the "quintessentially revolutionary text" (p. 66) even though it was written when the revolutionaries had been put to flight. Rose assimilates Wagner's racism into a questionable notion of "revolutionism." Revolutionism is at once the defining intellectual stance of the Young Germans and Young Hegelians, a synonym for a hyperactive nationalism and the explanatory vehicle of the composer's development. By grandly operating far above the interplay of biographical and political events, Rose inflates the rather modest "revolutionism" of the Young Germans while skirting Wagner's actual revolutionary experiences.

Rose's accusatory style converges with a monumentalist impulse. He eschews the psychological, and rather than belittle Wagner, takes seriously, all too seriously, the Meister's pretensions to being a "prophet." There is no deconstruction, no search for subtexts; what we have is old-fashioned, police-blotter intellectual history. Wagner becomes not a case study of resentment, but rather is elevated to the status of the eternal anti-Semite, the German Haman, whose baleful tones will echo through the ages. The single-minded focus on Wagner's anti-Jewish obsession isolates anti-Semitism from its affiliated hatreds. The notion of the Jew as the wirepuller, the "enemy" behind the other enemies, requires the full screen, for it is invariably the other delusions that bring the anti-Semite to grief. But if Rose fails to connect Wagner's love-hate relationship with France to his anti-Semitism, he does succeed in presenting Wagner as the false pupil of Heinrich Heine.

Rose takes no enemies: musicologists, Wagnerologists, and equivocators are given short shrift. And for good measure, in an appendix he hurls thunderbolts at Israeli Wagner-idolators who believe that "time" might alleviate the Wagner problem. Rose's blast illustrates the degree the *Historikerstreit* has repolemicized the writing of German history. The effort to normalize the German past has prompted a renewed stress on the abnormality of German cultural traditions. The long-scorned "Luther-to-Hitler" perspective—updated by Rose in his preceding volume, *Revolutionary Antisemitism in Germany from Kant to Wagner* (1990)—has made a comeback with German unification and ethnic cleansing.

Peter Bergmann
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Peter Pulzer. *Jews and the German State: The Political History of a Minority, 1848–1933.* (Jewish Society and Culture.) Cambridge, Mass.: Blackwell. 1992. Pp. xiii, 370. $49.95.

Over the last twenty years Peter Pulzer has published a number of stimulating and important essays that have become classics in the historiography of German Jewry. These essays, expanded and revised for this volume, produce a fresh and illuminating historical analysis of the political identity of the Jews in Germany from the early nineteenth century to Hitler's coming to power. Although professing that Jews or persons of Jewish descent could be found in diverse political parties and ideological organizations in Germany, Pulzer observes a distinctive pattern of identification with left-wing liberalism. Except for the brief period of 1866–78, when the political aspirations and

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values of Jews and gentiles in the commercial and professional middle classes coincided, the political attitudes of Jews differed notably from those of other Germans of the same social class.

For an explanation of the left-of-center and "asymmetrical" political stance of the Jews, Pulzer points to peculiarities in the occupational distribution of Jews in the German economy and, to a greater extent, to a permanent consciousness that the "Jewish Question" had not disappeared from German public debate. At the heart of this continuing debate over the civic equality of Jews was the problematic relationship of citizenship and nationality in Germany. The law of 1869 guaranteeing equality of rights to all German citizens regardless of religious faith did not eliminate discrimination against Jews in appointments to public office or settle the debate about the appropriate place of the Jew in German society.

In his analysis of the developments through which liberalism became the political home of the Jews, Pulzer discusses the significance of the revolution of 1848 in Jewish collective memory and the high degree of political integration achieved by the Jews during the era of National Liberal ascendancy (1866–78), when an unprecedented number of Jews were elected to the Reichstag and state parliaments and were appointed to the judiciary and university professorships. Bismarck's alliance with the Conservatives after 1878 ended the possibility of further liberal reforms. When the National Liberals split, most Jewish politicians in the party joined secessionists critical of Bismarck's policies and formed the Progressive Party with other Left liberals in 1884. From that time on, most Jewish politicians and journalists and the greater part of the Jewish population were in the camp of the politically oppositional and patriotically loyal Left liberals. In the years before World War I and in the 1920s, more and more Jews were drawn to the Social Democratic Party. Pulzer assesses the extent of Jewish support for the Social Democrats more accurately than other scholars have and points out the disparity between the large number of Jewish intellectuals who were prominent in the party's leadership and press and the slower growth of the party's Jewish electoral support.

During the period of the Weimar Republic, Jews remained faithful to the Left liberals—now organized as the German Democratic Party—despite the precipitous decline in the party's electoral strength and political influence after 1919. This loyalty did not falter when the German Democrats attempted to rescue liberalism by merging with the more conservative and nationalist *Jungdeutscher Orden* to create the German State Party in 1930. This strategy failed to halt the flight of middle-class voters to the National Socialist movement. Here Pulzer's treatment of the political dilemma of Jewish liberals is too detached and could be more attentive to their perceptions, thinking, and feeling. From his statistical study of electoral votes in Berlin, Frankfurt am Main, and Hamburg from 1930 to 1932, Pulzer reveals that "a significant proportion of the Jews clung to the German State Party until a very late stage" (p. 294). Only after the National Socialist vote increased alarmingly in the Prussian *Landtag* election in April 1932 did Jewish liberals in the Reichstag elections in July and November 1932 turn to the Social Democratic and Catholic Center parties as a more effective means of defending the republic.

For the average Jewish voter, Pulzer contends, "Jewish considerations" did not have "high priority" (p. 146). Jewish electoral support for the Progressives and later the German Democrats was not based on any special attention to Jewish interests. Tensions marked the relationship between Jews and the Left-liberal parties in the empire as well as in the republic. In the Wilhelmine era the Left liberals opposed anti-Semitism "sometimes conscientiously and sometimes negligently"; most Jews supported the party "simply because its advocacy of the rule of law, of careers open to talents and of economic liberty corresponded with the kind of society which Jews had the most reason to confide in" (p. 138). Stating later in his analysis that "the fight against anti-Semitism and discrimination could take place only within the framework of Liberal politics, and Left-Liberal politics at that" (p. 330), Pulzer introduces another reason that could be incorporated into his explanation more extensively. From the 1890s to the end of the Weimar era, the expediency of Jewish support for Left-liberal parties was the subject of considerable debate within the Jewish community. Pulzer does not relate the arguments made by the critics and defenders of this alignment in those times, but his book clarifies admirably a point that was often lost in the criticism of Jewish liberals' dependence on one party. Their liberalism was defined in terms of a particular kind of society as much as a political party.

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ERNST ENGELBERG. *Bismarck: Das Reich in der Mitte Europas*. Berlin: Siedler. 1990. Pp. xiii, 731. DM 58.

This volume completes the work Ernst Engelberg began with *Bismarck: Urpreusse und Reichsgründer* (1985, reviewed in *AHR*, 94 [1989], 167–68). Published simultaneously in East and West Germany, the first volume was notable for the absence of ideological distortions. A lifelong communist who had suffered for his beliefs under the Nazis, Engelberg had no need to demonstrate his orthodoxy at the cost of historical accuracy and balanced judgment. This volume is a fitting companion to the first.

That biographers must have some empathy for their subjects if they are to see the "I in the Thou," as Wilhelm Dilthey put it, is one of the aphorisms of the genre of biography. Engelberg's empathic understanding is evident in both volumes wherever he

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considers Bismarck's character and personality. A good example in this volume is his description (pp. 352–58) of the conflict that arose between Bismarck and his oldest son in 1881, when Herbert announced his desire to wed princess Elisabeth von Carolath-Beuthen, whose divorced status and familial connections made her unacceptable to the father. Herbert finally yielded when Otto threatened suicide and disinheritance. Engelberg's account is written with great sensitivity to the moral drama involved in this triangle and the emotional cost to all concerned.

But surely Engelberg goes too far in endorsing John Lothrop Motley's rather hyperbolic judgment of Bismarck (pp. 46–48). Motley, who had known Bismarck as a student at Göttingen and Berlin and visited him a few times in later years, remarked that his Junker friend was not one to exaggerate his power to shape events, was lacking in conceit and false pride, and understood the limits of human wisdom in the decision-making process ("Never lived a mortal so unaffected as he"). Engelberg concludes that Bismarck, because he looked on his profession as "a business" or "industry," could act politically with sobriety and objectivity. Although Engelberg recognizes that the chancellor's political struggles exacted a heavy toll on his nerves and physical health, he detects no consequences for his political judgment.

In Bismarck's management of German foreign policy Engelberg can find little to fault. The picture that emerges is that of a statesman guided by *Staatsräson*, who pursued the interest of state without regard to personal or ideological conceits. He dismisses the assumption of some historians and political commentators that Germany and Europe would have been better off had the German Reich never been created and Germany remained a confederation of sovereign states (p. 648). Throughout the book he appraises Bismarck's "policy of the balance of power" as beneficial to the interests of Germany and Europe. Bismarck viewed the Reich of 1871 as a "saturated state" with no need for further expansion and never looked on German-speaking regions outside its frontiers in central and eastern Europe as irredenta. He opposed the idea of preventive war ("We know where it starts but not where it ends") and strove to educate not only the governing elite but also the general public on the character and limits of the alliances he had constructed for Germany within the balance-of-power system. Engelberg notes, of course, that those alliances (with Russia and Austria-Hungary at the core) had the ulterior purpose of stabilizing the conservative social and political orders of central and eastern Europe against internal revolution, whether bourgeois or proletarian, but he appears to accept their value, even necessity, for the preservation of peace.

Engelberg's view of Bismarck's statesmanship in foreign policy tends to depict him as so masterful as to be almost incapable of error. This is evident in his account of the famous "Is-War-in-Sight?" crisis of early 1875, an episode that has puzzled many historians. Engelberg believes that the inflammatory newspaper articles that began the crisis, although written by subordinates, were deliberately used by Bismarck to test the solidarity of the Three Emperors League formed in 1872. Frictions between the members had caused him to fear that the league could not serve its chief purpose of isolating France. By successfully testing the alliance, he reinforced it, Engelberg believes. But the "test" actually engendered new fears of German military power and aggressiveness among European chancelleries and renewed sympathy for France. Furthermore, it gave Benjamin Disraeli and Aleksandr Gorchakov the chance to pose as the guardians of Europe's peace and enabled the French foreign minister Louis Decazes to paint Bismarck as an advocate of preventive war and inflict on him a humiliating diplomatic defeat.

The real explanation of the war-in-sight crisis is that Bismarck, worn out by his exertions and illnesses, blundered in his conduct of foreign policy, an event so rare that historians have been reluctant to recognize it for what it was. For once Bismarck violated one of his own principles of political behavior; he bluffed, the bluff was crude, and it was called. His wife Johanna's unpublished correspondence with sons Herbert and Wilhelm during this period (an underrated and underused resource) shows that Bismarck was so debilitated physically and mentally in the first half of 1875 that he had come to doubt his capacity to continue in office. His "resignation" of May 1875 was, unlike earlier and later ones (which had a coercive character), a serious decision. He wanted out, but Kaiser Wilhelm would not let him go and in the end Bismarck did not insist; instead he went on an "extended leave" that lasted many months.

Engelberg's account of the origins of the *Kulturkampf* (the struggle against political Catholicism begun in 1871 and fought mostly in the ensuing decade) is another instance in which he tends to exculpate Bismarck from blame. The author recognizes that this internal policy had a foreign political aspect in view of the international character of the church and the ambitions of its churchmen. In Engelberg's account, the struggle had its origin in the ultramontane pretensions and aggressions of the Catholic hierarchy; Bismarck's actions, he believes, were purely defensive in nature. With the help of liberal allies in Landtag and Reichstag he sought through the "May laws" to defend the state against clerical assault.

The origins of the *Kulturkampf* were many and complex; it was one of those cases where all sides—Catholics, liberals, the Prussian government—acted defensively in purpose but offensively in practice. The author devotes little space to the negative attitudes Bismarck had long displayed toward the Poles and Catholicism. On these subjects there was an emotional component in Bismarck's actions at variance with the cool, collected, objectively calculating image Engelberg has of his protagonist. The chancellor launched his "internal preventive war" against

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political Catholicism in June 1871 at a time when he was still exultant over his recent triumphs in both external and internal affairs. In this euphoric and hubristic mood he attempted to extend his recent "revolution from above" by seeking to level the remaining obstacles he perceived to be in the way of its consolidation.

In view of its eventual outcome, the *Kulturkampf* can only be regarded as a major mistake of Bismarckian policy. He exaggerated both the internal and external dangers of political Catholicism and misjudged the necessity for a counterattack by the state as well as the capacity of the state (of that era) to coerce the church hierarchy (from the pope himself down to the parish priests backed by their parishioners). And his attempts to assimilate the Poles by attacks on their leadership (Catholic clerics and landlords) and by Germanizing their schools, courts, and other government services merely galvanized the Polish population to protect its ethnic heritage. Nor did his government have much success in its efforts to Germanize other ethnic minorities—for example, the Danes residing in northern Schleswig and the French and pro-French inhabitants of Alsace and Lorraine—a subject that does not appear in Engelberg's account.

Engelberg credits Bismarck with a realism that enabled him to recognize and accommodate the needs of industrial capitalism and the rising force of popular nationalism. But his realism ceased where the social effects of industrial capitalism were concerned. On this point Engelberg's empathic understanding of the great chancellor becomes rather thin. He charges Bismarck not with malice but with ignorance and demagogy. Like most of his contemporaries in public life, Bismarck was blind to the needs of the industrial proletariat because he was ignorant of the writings of Ferdinand Lassalle, Karl Marx, and Gustav Schmoller (and other "*Kathedersozialisten*"), which could have helped him understand the dynamics of the capitalist system and its social effects. Bismarck's "uninformed attacks" on the economic and social problems of his time were predestined to failure (p. 222). His anti-socialist laws did not cripple the social democrats, and his social insurance acts did not win the hearts of proletarians.

In their accounts of internal politics of the 1870s and 1880s, historians are prone to concentrate on the parliamentary and electoral struggles between Bismarck and the liberal and center parties. Seen from this perspective, his chief opponents were Eduard Lasker, Rudolf Virchow, Eugen Richter, and Ludwig Windthorst. Certainly the sources show that Bismarck perceived them (particularly Lasker and Windthorst) to be his chief "*Gegenspieler*." Engelberg's choice (pp. 332, 389–90) for that role—August Bebel—would have surprised Bismarck himself. They rarely faced off in parliamentary debates, and Bismarck seldom mentioned Bebel in private conversation.

Those who labor in the busy shop of Bismarck-era research often find it easier to see the shortcomings than the virtues in someone else's workmanship. But we all know how difficult it is to master the details, great and small, of a life that spanned almost the whole of the nineteenth century and was intimately and influentially involved in political affairs for one-half of that period. Like all biographers, we wrestle with the problem of combining the chronological and analytical approaches to historical narrative. In many important instances Engelberg has accomplished those combinations with considerable skill. One example is his description of Bismarck's balancing act between Russia and Austria-Hungary during the Balkan crisis of 1878. Here and elsewhere Engelberg's talent at interweaving foreign and domestic events keeps the reader well informed about nearly simultaneous events on both fronts. The resulting tapestry often reveals new relationships and fresh insights.

What are we to make of Engelberg's seeming ambivalence toward his subject? Naturally none of Bismarck's conservative biographers have shown that tendency, but it is to be found universally among his liberal biographers. In Bismarck's lifetime middle-class democrats and even working-class socialists were of two minds about the chancellor and his career (pp. 528–31). To a degree Engelberg's ambivalence merely reflects his own socialist background. We have never lacked biographies written with a liberal or conservative bias; missing was one written from a socialist perspective. Now we have it.

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Detlev J. K. Peukert. *The Weimar Republic: The Crisis of Classical Modernity*. Translated by Richard Deveson. New York: Hill and Wang. 1992. Pp. xv, 334. $28.00.

At the time of his death at the age of thirty-nine, Detlev J. K. Peukert was widely recognized as the most imaginative and intellectually provocative of his generation of German historians. Not only was Peukert a prolific scholar whose work covered a wide range of topics and questions but also his scholarship demonstrated a freshness and a sensitivity to historiographical innovation that set it apart from most of what was being written in his field. His study of the Weimar Republic, first published in German in 1989, is a fitting tribute to his talents as a historian and a reminder of how much we have lost through his death.

Peukert's study of the Weimar Republic explores the perennially vexing question about the relationship between modernity and fascism. Peukert sees the Weimar Republic as a sort of crucible in which the various permutations of "classical modernity" were given the freedom to develop in whatever direction they might take. By "classical modernity," however, Peukert refers not just to the highly celebrated revolutions in art, literature, architecture, and music that

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characterized the first decades of the twentieth century but also more broadly to the equally revolutionary changes that took place in the areas of science, medicine, sexuality, urban planning, social policy, and industrial relations. As a "classic statement" of the developments in ideas and everyday life that define the "form of life that remains our own today," Weimar represented "a brief, headlong tour of the fascinating, and fateful, choices made possible by the modern world" (p. xiv).

Peukert's book, therefore, is anything but a narrowly conceived history of Weimar politics. In fact, Peukert deliberately eschews a detailed analysis of either the republic's founding or its demise in order to situate its history within the broader spectrum of continuity and change that defines modern German history. In this respect, Peukert is critical of the so-called *Sonderweg* thesis not because historians have begun to abandon the idea of a "normal model of modernization" against which the deviations of Germany's social, economic, and political development can be measured, but rather because it presents an overdetermined view of German history that robs the Weimar experience of much of its inherent value and significance.

Peukert's explanation of how and why Weimar failed reveals the extent of his intellectual debt to Max Weber. Here he employs a multicausal mode of analysis that identifies four distinct yet interrelated processes that combined to seal the fate of the Weimar Republic: the destabilizing effect of unremitting socioeconomic crisis, the steady erosion of the republic's political legitimacy, the reversion to authoritarianism by traditional elites intent on restoring the power relations of the Bismarckian Reich, and the rise of National Socialism as a dynamic, modern totalitarian mass party. The republic could have withstood any one of these processes but certainly not the combined effect of all four. This, in turn, created a situation in which the "keys to power" were handed over to the Nazis by "old elites who had been all too successful in destroying the Republic but too feeble to restore the pre-war order" (p. 280).

No review as brief as this can possibly do justice to the subtleties, complexities, or sophistication of Peukert's book. Suffice to say that his study of the Weimar Republic is an important introduction to the intricacies and contradictions of Weimar political culture that both challenges historical clichés about modern German history and provokes new thinking on Weimar's place in the modern world.

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DIETMAR KLENKE *et al. Arbeitersänger und Volksbühnen in der Weimarer Republik.* (Solidärgemeinschaft und Milieu: Sozialistische Kultur- und Freizeitorganisationen in der Weimarer Republik, number 3; Forschungsinstitut der Friedrich-Ebert-Stiftung, Reihe: Politik- und Gesellschaftsgeschichte, number 27.) Bonn: J. H. W. Dietz, for the Historische Kommission zu Berlin. 1992. Pp. 346. DM 98.

This volume is part of a series of works sponsored by the Historische Kommission zu Berlin that examine Social Democratic cultural and leisure organizations during the Weimar years. It consists of two monographs: a lengthier study of the *Deutsches Arbeiter-Sängerbund* (DAS) written by Dietmar Klenke and Franz Walter, and Peter Lilje's shorter account of the *Verband der Deutschen Volksbühnenvereine.*

The study of the DAS presents a detailed, judicious, and at times fascinating portrayal of workers' choral groups. Founded in 1908, the DAS provided a national organization for the numerous workers' choruses that had evolved locally over the preceding decades. Prewar workers' choruses were almost exclusively male and had a penchant not only for militant socialist music but also for many of the sentimental *Volkslieder* so dear to the Wilhelmine soul.

This configuration underwent substantial changes during the 1920s. The steady influx of women (from 9 percent of membership in 1914 to 32 percent in 1932) allowed many more mixed choruses to be founded, which in turn permitted the performance of more ambitious choral works. The shift toward artistically difficult music was further encouraged by the willingness of professional musicians to conduct workers' choruses after 1918, when the Wilhelmine stigmatization of socialist organizations lost its force. The musical "elitists" in the DAS promoted the performance of demanding pieces like those of Ludwig van Beethoven and Franz Schubert and advocated the composition of socialist *Tendenzlieder* that would be musically more sophisticated than the bombastic works inherited from the prewar era. Paul Lendvai and Hanns Eisler were the major providers of choral works that used the freer tonalities and dissonances of "advanced" music.

These changes spelled better performances but less fun. Membership in the DAS reached a peak of 263,700 in 1923, but declined thereafter. The choruses failed to recruit younger members, who seem to have followed the siren call of cinemas, sports, and commercial popular music. Moreover, the increasingly high standards of performance made rehearsals less convivial and more professional, and thus may have alienated a number of older stalwarts as well.

Klenke and Walter discuss at length the political dimensions of workers' choruses. The DAS had no official organizational ties to the Social Democratic Party (SPD), but most of its members belonged to that party or to the trade unions. In analyzing particular choral works, the authors underscore problematic traits of SPD culture that dated back to the Wilhelmine era. The best-loved political songs espoused class struggle and proletarian internationalism, but they also were replete with masculine and militant imagery and they evoked the coming revolution in visionary

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and religious terms. Furthermore, none of the works dealt concretely with current events or explicitly supported the Weimar Republic. By maintaining a revolutionary tone and forgoing direct acclaim of the republic, the DAS could harbor not only Social Democrats but also members of the Communist Party (KPD). Unlike some other leftist cultural and leisure organizations of the 1920s, the DAS was not torn apart by SPD/KPD animosities; KPD attempts to split the ranks of the DAS from 1930 to 1932 had relatively little effect. Indeed, the music specialists of the DAS actively encouraged the performance of works by Eisler, despite his vociferously pro-communist and antisocialist views.

Klenke and Walter have written a well researched and thoughtful monograph that argues for a differentiated approach to socialist culture in the 1920s. The DAS was more radical in tone than other SPD-oriented organizations of the period, but at the same time it developed a more elitist and professional attitude toward musical performance. Its relative hostility to newer forms of mass culture also distinguished it from other socialist groups and accounted for a loss of membership over time.

This volume concludes with Lilje's much shorter history of the *Verband der Deutschen Volksbühnenvereine*, founded in 1920. By the time the *Volksbühne* movement created this national organization, it had lost the radical impetus that had characterized it in the 1890s; it became essentially a means for lower-income groups to purchase reduced-price tickets for local performances. Since there already is extensive scholarship on the Berlin *Volksbühne*, Lilje forgoes discussion of artistic and ideological disputes in the movement; his study is more an organizational history and statistical survey, based on the official publications of the *Verband*. The work is valuable in shifting focus away from Berlin, but Lilje admits that many more detailed studies of local *Volksbühne* groups are needed to generalize about the movement as a whole. Data on the social composition of membership are still sparse, and extant documentation indicates only loose ties to the SPD and the trade unions. Lilje suspects, however, that further and more detailed studies will uncover closer connections.

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KARSTEN RUPPERT. *Im Dienst am Staat von Weimar: Das Zentrum als regierende Partei in der Weimarer Demokratie 1923–1930*. (Beiträge zur Geschichte des Parlamentarismus und der politischen Parteien, number 96.) Düsseldorf: Droste. 1992. Pp. 469. DM 78.

The premise of Karsten Ruppert's informative book is clear from its aptly chosen title: as the only party to serve in nearly every Weimar government, the (Catholic) Center Party deserves increased attention from students of German democracy. In improving our knowledge of government practice during the least tumultuous phase of the Weimar Republic, Ruppert has written more than a party history. Yet Ruppert refuses to draw broader implications from this story about the longer-term development of democracy in Germany.

A major theme of the book is the Center Party's progressive loss of confidence in the political relevance of its traditional mission. As the representative of a disadvantaged minority group, the Center Party was unaccustomed to governing; when pushed into this role under the republic, its religious self-understanding proved to be both a help and a hindrance. Its quintessential leader, Wilhelm Marx, was a "politician who lacked the political instinct" (p. 350); he was guided neither by ambition nor by a clear political agenda, but by a pious, duty-driven ethos of civil service and the golden mean. Although this trait facilitated the Center Party's transition to parliamentary government, it also rendered the party pliable and politically directionless. This problem, Ruppert suggests, became increasingly pronounced once the constitution's religious settlement (1919) moved other issues of governance clearly to the fore.

Each of those issues—hyperinflation, princely expropriation, polar coalitional swings, and the unexpectedly bitter opposition of Christian trade unionists to civil service salary increases in 1927/28—heightened the sense of foreboding within the party, and Marx was caught in the middle. Ruppert presents the best picture we have of Weimar's most frequent chancellor, whose prime qualification for the job was his comparable mediating experience within his sociopolitically diverse party. But as the colorless Marx coaxed and compromised from one coalitional crisis to another, he seemed committed to little beyond republican crisis management and a pro-clerical school bill. Whereas younger, more forceful politicians such as ex-Chancellor Joseph Wirth and Christian trade-union leader Adam Stegerwald tried to push the Center Party to prioritize and to systematize its secular agenda, Marx developed the party neither organizationally nor programmatically. A spent man, he gave way at the end of 1928 not to his vigorous vice chairman, Stegerwald, but to the hapless Monseigneur Ludwig Kaas, not, says Ruppert, because the explicitly religious element in the party had prevailed, but because the party was too divided to agree on any other course.

The Center Party's fundamental problem, writes Ruppert, stemmed not from its internal social tensions (which were nothing new) but from its inability to reconcile its subcultural or sectional identity with the expanded demands of its governmental role. Of course, the party's uniquely diverse constituency was itself a product of its sectional identity. But there is an important lead here that Ruppert does not quite exploit: since democracy meant that any sectional party was incapable of governing, the Center Party

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was handicapped because it at least tried. The essence of the party's situation was not that it was a subcultural relic, but that it was not. Because its sectional identity still mattered, the Center Party—unlike the later Christian Democratic Union—was prevented from turning its sociopolitical diversity (in which it was ahead of its time) into a governing advantage.

As Ruppert shows, Weimar's middle years were psychologically anything but golden. Unfortunately, he comments only intermittently on the significance of his wealth of detail. For this reason, his valuable book is unlikely to rescue political Catholicism from its still peripheral place in modern German political historiography.

NOEL D. CARY
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JAMES S. CORUM. *The Roots of Blitzkrieg: Hans von Seeckt and German Military Reform.* (Modern War Studies.) Lawrence: University Press of Kansas. 1992. Pp. xvii, 274. $29.95.

James S. Corum's book is a welcome addition to the literature on the interwar German Army (*Reichswehr*). The standard works in this area, such as Harold Gordon's *The Reichswehr and the German Republic* (1957) or F. L. Carsten's *The Reichswehr and Politics* (1966), have focused on the army's political involvement, its lack of support for the Weimar Republic, and its role as a "state within a state." Corum looks at the army's real mission, which was not to undermine the republic but to provide for Germany's national security under some very trying circumstances.

Corum explains in painstaking detail how General Hans von Seeckt, chief of the Army Command from 1920 to 1926, revived the army after its collapse in 1918. He began with a comprehensive review of the lessons to be learned from World War I. Based on those findings, he drew up a new series of tactical manuals (such as *Leadership and Battle* [1921]), which thoroughly reformed the army's training procedures. The result was a small yet high quality "army of commanders" (*Führerheer*), in which even the lowliest enlisted man was prepared to assume officer's rank when political conditions enabled Germany to expand its army from its Versailles-imposed limit of 100,000 men.

The true strength of this book is the research. Corum has assembled many and varied sources—German *Truppenamt* (General Staff) records, tactical manuals, Seeckt's own writings, and a comprehensive selection of hard-to-find military literature from the 1920s. He also includes a wonderful set of rare photographs, dummy tanks and all. He delves far deeper than the usual list of standard works to provide insightful analysis of lesser known military writers: armor theorist Ernst Volckheim, for instance, or Helmut Wilberg's theories on air warfare.

Armed with this broad range of sources, Corum engages enthusiastically in controversy. Previous historians have tended to view Seeckt as an important figure, but not a particularly innovative one. Corum disagrees. His Seeckt is an original thinker, "one of the most important military thinkers of the twentieth century" (p. xiii). Seeckt saw war above all in terms of mobility; a highly mobile, flexible infantry augmented by the use of modern cavalry and air power was the army of the future. The mass armies of the world war had been incapable of true maneuver, and thus incapable of victory.

Corum also takes on several common "misconceptions" regarding German armor development, for instance that it was largely the work of Heinz Guderian, who had to struggle against a reactionary High Command in implementing his new theories. Corum argues forcefully that this is a fable. The source for this widely held belief is Guderian himself, especially his self-aggrandizing autobiography *Panzer Leader* (1952), backed up by a handful of perhaps apocryphal anecdotes. Likewise, Corum argues, the notion that *Blitzkrieg* arose out of ideas imported from Great Britain, especially in the writings of Basil Liddell Hart, is equally unsupportable. Only a handful of Liddell Hart's articles were translated into German during this time; few appeared in the leading German military journal, the *Militär Wochenblatt*.

Corum does successfully debunk Guderian's claims to be the father of armored warfare, but at times he overstates Seeckt's role. His claim of a "documented enthusiasm for motorization" on the part of Seeckt and the German High Command is debatable. Based on Seeckt's own writings (his article, "Modern Cavalry" [1927], for example) or his yearly *Observations of the Chief of the Army Command*, one could argue just as easily that he had a "documented enthusiasm" for horse cavalry. Discussion of armor takes up little space in Seeckt's writings, making him more of a transitional figure than a true innovator in this area. And in focusing solely on the Seeckt years, Corum underemphasizes those who came later. There is little discussion of what followed Seeckt's resignation, the change of course in German military planning from preparing a *Führerheer* for the future to a more realistic emphasis on national defense with the forces Germany had at its disposal (the *Landesverteidigung* concept).

But these quibbles are offered more as points of discussion raised by Corum's analysis than as criticism. This is a superb book, both scholarly and highly interesting, and a major contribution to the field.

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DENNIS SHIRLEY. *The Politics of Progressive Education: The Odenwaldschule in Nazi Germany.* Cambridge: Harvard University Press. 1992. Pp. x, 277. $34.95.

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According to Dennis Shirley, Paul Geheeb found himself in 1933 in the midst of a savage culture war, and no one could have been more ill-suited than the strangely eccentric headmaster of Odenwaldschule to do battle with the Nazis. Geheeb's school not only catered to the wealthy but it also encompassed practically all of the categories of humanity the Nazis despised most: Jews, pacifists, socialists, humanitarians, egalitarians, feminists, and liberals. The possibility that a tiny, struggling, private academy, with a wealthy Jew as its primary benefactor, could survive the institutionalized hatred of the Nazis was remote indeed. Furthermore, Geheeb had already lost a series of educational battles against far less ruthless antagonists.

Even before founding the Odenwaldschule in 1910, Geheeb had established himself as a courageous and imaginative educational reformer, although his credibility was marred by an inability to maintain steady employment. The turning point in his career came with his marriage to Edith Cassirer, the daughter of a Berlin cellulose manufacturer who overcame his initial dislike of Geheeb and financed his educational endeavors.

Although possessing no coherent program when the school opened its doors in 1910, Geheeb eventually put in place a distinctively humanistic curriculum that included student self-government, egalitarian relations between the sexes, ethnic and religious equality, and a deeply held reverence for life in all of its manifestations. The Odenwaldschule permitted no hierarchial arrangements, nor were children given grades or segregated by grade levels. Geheeb's refusal to permit the religious rituals of any orthodox body angered some, as did his sponsorship of nude jaunts in the forest, which he labeled "air baths" (see the photographs following p. 133). It was probably inevitable that many in rural, conservative Hesse would refer to the Odenwaldschule as a "communist, Jew school" (p. 92).

Geheeb subordinated intellectual reflection to a developmental process that formed students into an educational community. Upbringing, in its broadest meaning, took precedence over cognitive development. "Become who thou art" was the motto of his curriculum of individuation (p. 209). Goethe and Fichte comprised the most prominent members of an "invisible faculty" (p. 45), while those present included many bright, young academicians who joined the faculty as *Mitarbeiter*. Prominent lecturers, such as Martin Buber and Georg Fuchs, appeared on occasion also. Geheeb's influence dwarfed all others, however, even though his eccentricity and cultish proclivities added to the school's troubles.

When President Paul von Hindenburg appointed Adolf Hitler chancellor of Germany on January 30, 1933, life at the school changed forever. The sixty-three-year-old Geheeb first attempted a strategy of accommodation. Shirley even accuses him of sycophancy. Initially, Geheeb appeared to score points by removing some of the practices that appeared most obnoxious to the Nazis. He also employed several Nazi faculty and aligned the Odenwaldschule with Hitler's struggle against communism. Cassirer encouraged Geheeb to go further, believing that, if placated, Hitler would leave the school in peace. The incongruity between the old and the new proved too great, however, and Geheeb ultimately resigned. He had tried to bend but he would not "surrender his soul to the devil" (p. 133).

In the summer of 1934 Geheeb immigrated to Switzerland where he founded the École d'Humanité. Meanwhile, Heinrich Sachs assumed leadership of a cleansed Odenwaldschule (p. 193), which he led until his removal in September 1945. Although Sachs shielded Jewish and dissident children from arrest and preserved a measure of the school's legacy, his reputation was tarnished by efforts to appease the Nazis that went far beyond any undertaken by Geheeb. Nor was Sachs able to restore his relationship with his former mentor after the war. Shirley points out that the heroism of Geheeb and the collaboration of Sachs, although real, represent overdrawn caricatures. Each man displayed both heroism and cowardice, and each man resisted the evil of Nazism in his own way. Passing judgment on their behavior from a vantage point of safety is risky, and Shirley wisely leaves that judgment to the reader.

This is a balanced account, written with compassion and an enviable command of the sources. Those interested either in contemporary history or comparative education should take note of this work. It fills an important gap in the scholarship of the period.

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David C. Cassidy. *Uncertainty: The Life and Science of Werner Heisenberg*. New York: W. H. Freeman. 1991. Pp. xii, 669. $29.95.

David C. Cassidy has written an absorbing and provocative biography of Werner Heisenberg. The short title is meant to suggest not only Heisenberg's considerable achievements in modern quantum mechanics (the uncertainty principle) but also uncertainty about the ethical and moral responsibility of an influential German physicist who chose to remain in Germany and to direct German atomic research during the 1930s and early 1940s. The volume is an important complementary piece to John Heilbron's study of Heisenberg's older colleague Max Planck (*The Dilemmas of an Upright Man: Max Planck as Spokesman for German Science* [1986]), with Cassidy taking a harsher judgment than Heilbron about the culpability of his protagonist.

Heisenberg was a member of an academic family. His father held the chair in Byzantine philology at Munich and his mother's father was rector at the Maximilien Gymnasium where Heisenberg matricu-

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lated in 1911. At the gymnasium, he became a member of a "Preparedness Association," which was called to duty in the spring of 1919 on the side of the "whites" against the "reds" in suppression of the soviet in Munich. Heisenberg's Preparedness Association became the Gruppe Heisenberg within the New German Pathfinders (*Neudeutsche Pfadfinderschaft*) in the early 1920s.

A main theme in Cassidy's book is the centrality of this youth group to Heisenberg's life from the 1920s through the early 1930s. Whenever in Munich, Heisenberg spent his weekends with these young men, often at their Bavarian ski hut, to which he invited Niels Bohr in the early 1930s. The group's activities included music and poetry, swimming and hiking, as well as discussions focused on the moral decadence of a modern Germany in need of "truth and uprightness" (p. 69). Cassidy's judgment is that this group of well-placed and well-educated young men ill-served the needs of political and social reform by, literally, heading for the hills. In Heisenberg's case, when not in the hills, he sought a "plane of higher existence" (p. 71) in theoretical physics.

Heisenberg first entered the University of Munich in 1920, where he quickly became one of Arnold Sommerfeld's protégés, completing his first publishable physics paper after only eighteen months as a student. In the winter of 1922–23, Heisenberg visited Max Born's Institute for Theoretical Physics in Göttingen and, in 1924, Bohr's institute in Copenhagen where he became Bohr's assistant in 1926. Cassidy describes Heisenberg's development of a mathematical program of replacing mechanically conceived electron orbits with matrix elements. In a paper he wrote in 1927 he argued that one cannot measure the position and momentum of an electron simultaneously. This was taken to mean that there is an irremediable uncertainty in our knowledge about individual events in the subatomic world. This principle, along with Bohr's principle of wave-particle complementarity and Born's statistical interpretation of Erwin Schrödinger's wave equation, became known as the "Copenhagen Interpretation," hotly disputed by Albert Einstein but by few others after 1927.

Another important theme in Cassidy's analysis is the preoccupation of Heisenberg the physicist with success, both in the philosophical pragmatic sense of getting the right numbers (and not worrying about physical or realist meaning) and in the professional sense of making a name for himself and furthering his academic career. Cassidy is skeptical that Heisenberg had a deep philosophical bent of mind. And Heisenberg was wildly successful. In 1927, at the age of twenty-five, he received a chair at Leipzig. In 1932, at the age of thirty-one, he received a Nobel Prize.

But what of his complicity in German physics under the National Socialist regime? Heisenberg watched while many of his immediate colleagues and friends left Germany amid indignities, threats, and violence. He sought Planck's counsel and concluded that physics must be preserved in Germany. In 1937, the same year that he married Elisabeth Schumacher, he was attacked in the SS weekly as a "white Jew," but, after threatening to leave Germany, he received reassurance from Heinrich Himmler that there would be no further attacks. This episode, Cassidy suggests, reinforced Heisenberg's decision to remain in Germany despite the ease with which he could have moved to an American university.

The details of Heisenberg's discussions about atomic fission and atomic weapons, particularly with Enrico Fermi in 1939 and Bohr in 1941, have often been told. Cassidy notes that émigré scientists and the Office of Strategic Services considered kidnapping Heisenberg in the early 1940s. Cassidy details Heisenberg's role in German atomic fission research in an account that can be supplemented by recent books by Heisenberg's widow and by historian Mark Walker. The concluding picture is one of Heisenberg's feverish determination in the last months of the European war to demonstrate the explosive power of atomic fission by reaching critical mass. After he was captured by the Allies, Heisenberg was surprised and chagrined to find that the Americans had developed the theory and the bomb. In a joint statement with fellow German scientists, he claimed that they had chosen not to pursue research in atomic fission, a claim Cassidy finds "preposterous." According to General Leslie Groves, Heisenberg's close friend Carl Friedrich Weizsäcker claimed that "If we had wanted Germany to win the war we could have succeeded" (p. 507). Not without reason, Cassidy is deeply troubled by Heisenberg's determination to establish that his war research "should be seen not only as ethically untainted but also as greatly successful" (p. 514). This is a fine biography, well worth reading by historians and non-historians alike.

MARY JO NYE
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RONALD HEADLAND. *Messages of Murder: A Study of the Reports of the Einsatzgruppen of the Security Police and the Security Service, 1941–1943*. Rutherford, N.J.: Fairleigh Dickinson University Press or Associated University Presses, London. 1992. Pp. 303. $45.00.

CHRISTOPHER R. BROWNING. *Ordinary Men: Reserve Police Battalion 101 and the Final Solution in Poland*. New York: HarperCollins. 1992. Pp. xxii, 231. $22.00.

The *Einsatzgruppen* (EGs), the Nazis' mobile police units (linked to the Security Police and Security Service) that carried out mass executions on a vast scale in the conquered Soviet territories, helped to establish their own infamy by issuing reports that are the subject of Ronald Headland's book. The best-known series of reports was called *Ereignismeldungen UdSSR*, translated by Headland as Operational Situational Reports (USSR) and later continued under

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different titles. Headland also includes and analyzes other surviving individual reports by EGs and their company-sized subdivisions, the *Einsatzkommandos* (EKs). These police units counted and recounted their mass executions and described political, social, and military conditions wherever they were located. The recording of monstrous crimes, like the crimes themselves, was turned into systematic and bureaucratic routine.

The EGs sent information to Berlin by radio, teletype, and courier; the EKs normally reported to their respective EGs but occasionally sent their own data to Berlin, too. The compiler and editor of the information and the distributor of the *Ereignismeldungen* was Group IV A 1 in the Reich Security Main Office (RSHA), later called the command staff. Heinrich Müller had personal control over the structure and tone of the reports. Each report contained a distribution list of high officials and offices beyond the confines of Himmler's SS and police empire that tended to expand over time. No other surviving set of documents provides such plentiful contemporary documentation of mass murder.

Headland wisely cautions the reader against blind reliance on these reports. Not only did RSHA headquarters have an interest in excluding critical comments and playing down the role of competing organizations but also the EG chiefs themselves sometimes avoided or omitted issues and events, alluded to fundamental orders only in passing references, or used euphemisms. Other documentation and postwar trials, infrequently used by Headland, have established the facts about some mass killings in considerably more detail than is contained in the EG reports. This book is therefore a study of what can be gleaned internally from the EG and EK reports rather than a study of the EGs and EKs themselves. Helmut Krausnick and Hans-Heinrich Wilhelm's fine study, *Die Truppe des Weltanschauungskrieges* (1981), provides more information from a broader range of sources about the latter.

The tenor of the reports and the nature of EG activities suggest to Headland that the killing of all Soviet Jews was a recognized Nazi goal from the beginning of the German invasion, although there were variations in the way that plan was carried out. Headland plausibly maintains that EG leaders and officers shared the typical ideological characteristics of SS men but were also influenced by career considerations and peer pressure.

If Headland demonstrates what can be done with contemporary reports, Christopher R. Browning shows what can be done with postwar recollections of perpetrators. Browning too is concerned with the psychology of mass murder carried out not in impersonal, factory-style extermination camps, but face to face.

Browning focuses on the rounding up and shooting of Jews carried out by Reserve Police Battalion 101 in eastern Poland during 1942 and 1943. He sets this innovative microstudy in a number of contexts: the intense wave of mass murder in Poland from March 1942 to February 1943, the history of the Order Police, and the use of Reserve Police battalions (composed of men too old for regular military service) as executioners and supervisors of non-German executioners.

Browning chose Battalion 101 partly because of the availability of rich interrogations of nearly half of the battalion members during war crimes investigations and trials from 1962 to 1972. The most unusual element in the battalion's history is that the commanding officer, Major Wilhelm Trapp, offered his men a choice before the first killing action: anyone who did not feel up to the job could remove himself without facing punishment. (Elsewhere, some men opted out of killing, but rarely were they explicitly offered an opportunity to do so.) This voluntarism allows Browning, reconstructing on the basis of the interrogations, to divide the battalion members into three categories: a nucleus of increasingly enthusiastic killers, a larger group of policemen who performed their duties when asked, and a small group of refusers and evaders (p. 168).

In highlighting this battalion's geographical origin (Hamburg, the least Nazified of the big cities) and the age of the policemen, Browning suggests that these policemen were unpromising raw material for mass murderers of Jews (p. 48). Yet despite some dissenters, they got the job done. We are told, however, that about 25 percent, a "surprisingly high percentage" (p. 48) of the men, had become Nazi Party members by 1942; we are not given the percentage of SS members in the battalion. So perhaps there is some uncertainty about Browning's view.

Citing Stanley Milgram's famous experiments, Browning maintains that obedience to authority and conformity (peer pressure) were more important motivators than Nazi ideology. Based on his examination of the surviving propaganda pamphlets for the Order Police and the men's own testimony, he discounts the effectiveness of indoctrination of the Order Police but concedes that the men were not immune to the influence of the era (p. 168).

Anyone who has used war-crimes interrogations knows that even when the suspects are trying to recall and describe events honestly, some subjects are handled much better than others. It may have been impossible (and was uncomfortable) for policemen and former policemen in the 1960s—and many continued their careers after 1945—to resurrect the extent to which they shared the ideology and atmosphere of Nazi Germany. The historian must impose judgment and apportion the influence of various causes, and neither Browning's sources nor his bottom-up approach are neutral here.

This book provides a readable, detailed picture of mass murder carried out by a segment of the police sorely neglected in the literature on the Holocaust. It is welcome and thought-provoking reading even for

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those who find Browning's universalistic conclusion (p. 189) slightly forced: "If the men of Reserve Police Battalion 101 could become killers under such circumstances, what group of men cannot?"

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KLEMENS VON KLEMPERER. *German Resistance against Hitler: The Search for Allies Abroad, 1938–1945*. New York: Clarendon Press of Oxford University Press. 1992. Pp. xvi, 487. $49.95.

The most vivid memory I have of the discussion of the anti-Nazi Resistance in my first undergraduate course on German history is of the instructor concluding that for the sake of postwar Germany's prospects for political and moral rehabilitation, an effort to overthrow the Hitler dictatorship such as culminated in the July 20, 1944, bomb plot was necessary. Yet it was also better that the assassination attempt failed because otherwise a new stab-in-the-back legend emanating from the Führer's "untimely" death would have bedeviled the German recovery. Klemens von Klemperer, in quoting conspirator Henning von Tresckow's famous words the day after the attempt, just before committing suicide ("I have reason to hope that, for our sake, God will not destroy Germany"), would perhaps endorse the former proposition (p. 385). Klemperer's nuanced and persuasive account of the role foreign policy objectives and their futile pursuit played in the ultimate tragedy of the Resistance, however, rejects the cynical verdict that J. W. Wheeler-Bennett voiced in a memorandum to Prime Minister Winston Churchill and Foreign Secretary Anthony Eden on July 25 (subsequently echoed in their public and private statements on the unsuccessful coup): it was "to our advantage" that the Nazi purge of the conspirators should continue, "since the killing of Germans by Germans will save us from future embarrassments of many kinds" (p. 387). Among the latter, Klemperer demonstrates, was an increasing focus by the German resisters, in common with many adherents of other Resistance movements across the Continent, on a made-in-Europe peace based on some form of European union transcending national frontiers and interests. This vision stood in too-stark contrast to Allied war aims, shaped primarily by the two emerging extra-European superpowers (the United States and the Soviet Union), for there to be a meeting of minds with the opposition inside Germany. A similar repudiation awaited the antifascist forces in the erstwhile Axis-occupied countries following their liberation.

Much earlier, however, Hitler's domestic conservative opponents—with whom Klemperer is mainly concerned, rather than Hitler's enemies on the Left and in exile—had managed to alienate influential opinion in London and Washington. Spokesmen like Carl Goerdeler and Adam von Trott zu Solz, besides appearing compromised by past or even continuing official connections with the Nazi regime (almost indispensable to travel abroad), persisted until late in the war in advocating unacceptable territorial claims vis-à-vis Poland, Czechoslovakia, Austria, and France that they or their equally indispensable military supporters hoped to salvage for a post-Hitler Reich. But Klemperer insists that more fundamental moral and indeed religious motives inspired the members of the German Resistance, best represented by counts Helmuth James von Moltke and his ecumenical Kreisau circle, as well as Claus von Stauffenberg, the would-be assassin. This helps explain why churchmen, above all Bishop George Bell of Chichester and the Dutch secretary-general of the incipient World Council of Churches, Willem Visser't Hooft (the two men to whom the book is appropriately dedicated), constituted the Resistance's most receptive foreign contacts. These and other clerics in Scandinavia and the Netherlands grasped the essential nature of the spiritual "war behind the (conventional) war" that informed the "treason" of their German friends. Ironically, the ideological factor was also appreciated by the Anglo-American intelligence counterparts of Admiral Wilhelm Canaris and his *Abwehr*, alas (a term Klemperer often employs) to no avail.

Aside from misrepresenting in the text the German Resistance's part in saving Danish Jewry (p. 333) and Willy Brandt's alleged denunciation of it as opportunist (p. 392)—both interpretations contradicted by information buried in endnotes—this is an impressively researched and finely balanced summation of an important subject.

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FRANK STERN. *The Whitewashing of the Yellow Badge: Antisemitism and Philosemitism in Postwar Germany*. Translated by WILLIAM TEMPLER. (Studies in Antisemitism.) New York: Pergamon, for the Vidal Sassoon International Center for the Study of Antisemitism (SICSA), The Hebrew University of Jerusalem. 1992. Pp. xxv, 455. $60.00.

Frank Stern in this volume traces the evolution of German policies and public opinion regarding Jews during the Allied occupation and the first four years of the Federal Republic. He argues that the rise of philo-Semitism should not be construed as a genuine manifestation of repentance for Nazi genocide. Neither should the reemergence of anti-Semitism in postwar Germany be taken as a simple reversion to Nazi racism. Instead, he contends that both represent related responses to the contradictory economic, intellectual, and political forces that rapidly transformed a disgraced, defeated, and devastated former enemy into a reconstructed and rehabilitated Cold War ally.

Drawing on an impressive array of sources, includ-

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ing memoirs, the reports and public-opinion surveys prepared by American occupation agencies, postwar polls conducted by the Allensbach and Frankfurt institutes, and Lutz Niethammer's oral history project on the Ruhr, Stern discerns several recurring themes in German reactions to the Holocaust. Although perfunctory general regrets about the atrocities committed by the Third Reich and a particular revulsion to the violence of *Kristallnacht* were expressed, the escalation from discrimination against German Jews to their deportation and extermination were usually remembered as an amorphous process that ended when the Jews "just disappeared" (pp. 220–37). The majority of Germans resented the deprivations and injustices they suffered under Allied rule and evaded a sense of collective guilt for the Holocaust by dismissing it as the secret policy of a criminal regime.

Philo-Semitism, however, entered into the public policies and pronouncements of postwar German leaders like Konrad Adenauer as part of a deliberate effort to prove their democratic metamorphosis to the Allies and thereby quickly attain the termination of Allied control, the establishment of German sovereignty, and financial aid from the West. They demonstrated their change of heart by denouncing anti-Semitism and Nazi genocide, accepting—although rarely promoting—the repatriation of returning German Jews, stressing their cordial personal ties to Jews in the past and present, idealizing the Jewish contribution to German culture, and supporting Israel. These German elites developed a terminology to refer to Jews only in positive ways. Religious reconciliation with the Jews became the special mission of societies for Christian-Jewish Cooperation, whose popular impact tended to be restricted to organizing an annual "Week of Brotherhood." Official philo-Semitism culminated in West Germany's approval of the Luxemburg Agreement in 1952 to provide restitution for surviving Jewish victims of Nazi persecution. The agreement was ratified by the *Bundestag* only because many of its members abstained or absented themselves from the vote.

Stern convincingly demonstrates that traditional anti-Semitic stereotypes were fully compatible with this new philo-Semitism. The image of Jews as wealthy capitalists was retained, as philo-Semites believed their approach would attract substantial Jewish investment in the new Germany. Other German politicians hoped that their favorable attitudes toward Jews would persuade the powerful German-Jewish advisers and American Jewish lobby, who allegedly influenced the Allies to pursue punitive occupation policies, to relent and endorse German recovery. Sharp distinctions were often drawn between the good German Jews and the bad Jewish refugees from Eastern Europe, who were commonly blamed for the black market, crime, and shortages in the immediate postwar years. While emphasizing their democratic and philo-Semitic credentials to Allied officials and foreign dignitaries, German politicians undermined denazification policies and minimized the significance of postwar anti-Semitic demonstrations and incidents in Germany.

After an extensive analysis of the period between 1945 and 1953, Stern concludes his book with a much too cursory chapter on the Federal Republic since then. Although he correctly asserts that the foundations for subsequent German-Jewish relations were laid in the period that forms the focus of his study, he simplifies the last two decades as ones in which even the limited philo-Semitism of the Christian Democratic years was eroded, paving the way for the disturbing outbreaks of anti-Semitism and racism that have erupted since German reunification. It undoubtedly has become more acceptable to express anti-Semitic opinions or to "normalize" the history of the Third Reich. Nonetheless, there also have been important positive developments, such as Willy Brandt's dramatic gesture of atonement at the Warsaw Ghetto memorial, the commemorations of the fortieth anniversary of *Kristallnacht*, the response to the showing of NBC's "Holocaust" and its impact on the abolition of the statute of limitations for murder, and President Richard von Weizsäcker's speeches about the necessity for Germany to assume historic responsibility for the Holocaust. The recent, albeit belated, mass demonstrations against racism represent forces that are still contending with the negative legacy Stern depicts in his book.

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RANDOLPH STARN and LOREN PARTRIDGE. *Arts of Power: Three Halls of State in Italy, 1300–1600*. (The New Historicism: Studies in Cultural Politics, number 19.) Berkeley and Los Angeles: University of California Press. 1992. Pp. xi, 374. $65.00.

Randolph Starn and Loren Partridge deal with three familiar Italian monuments from an unfamiliar point of view. They interpret Ambrogio Lorenzetti's allegories in the Sala dei Nove of the Palazzo Pubblico of Siena (1338–40), Andrea Mantegna's frescoes in the Camera Picta of the Palazzo Ducale in Mantua (completed in 1474), and Giorgio Vasari's murals in the Sala Grande of the Palazzo Vecchio in Florence (ca. 1565) as exemplars of three kinds of regime: republican, princely, and that which the authors aptly designate "triumphalist." With these models, they analyze "art as a form of power . . . [and] the operations of power in the forms of art" (p. 6). The first two sections are by Starn, that on Florence is by Partridge, with an introductory section on triumphalism contributed by Starn. The authors consider each monument in the broadest cultural context—to elucidate the practical and symbolic use of these rooms by their contemporaries—and in relation to the actual context, that is, the site and original furnishings.

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Discussing the Sala dei Nove, Starn rightly underscores the significance of the room's inscriptions, transcribed and translated by Ruggero Stefanini in the book's first appendix (pp. 261–66). I disagree with Starn's tentative proposal that Lorenzetti himself was the author of these inscriptions (pp. 33–34); it seems more likely that Lorenzetti had a collaborator who provided these texts. But the question of authorship is peripheral to Starn's discussion, which is informative and far-reaching, ranging from the literary history of this type of "allegorical 'vision poem'" (p. 35) to an iconographic analysis of Lorenzetti's spatial illusionism (pp. 48 and following).

Mantegna's Camera Picta served as "audience room and a bed chamber" for Marchese Ludovico Gonzaga, and possibly also as a repository for his "personal archives . . . the seals of subject towns and castles and the keys to the holiest site in Mantua" (p. 85). The fresco decoration is in keeping with these uses, simultaneously personal and official. Thus, the murals are like a family photo album set in a larger political context: "the family group is a pictorial genealogy . . . framed at each side by the activity of courtiers" (p. 95).

Mantegna signed the cycle twice, with the formal dedicatory signature, dated 1474, and the discreet one hidden on the letter in Cardinal Francesco Gonzaga's hand. These two written records of Mantegna's achievement are joined by a visual one, the self-portrait in the painted border, as noted by Rodolfo Signorini, who also discovered the name on the letter (p. 106). Such paired signatures, juxtaposing the artist's likeness and his name, were not unique. One recalls young Ghiberti's bland face in the framework of his first doors for the Florentine Baptistry, and his more idiosyncratic self-portrait years later, near his signature on the Gates of Paradise.

In the final section of the book, Partridge considers Vasari's Sala Grande from two interrelated points of view: as the visualization of Duke Cosimo de' Medici's regime and as a critical element of the *mise en scène* for the celebrations of the marriage of his son, Francesco, to Giovanna of Austria. Giovanna's entry into Florence in 1565 "came to a climactic end" in the sala (p. 155), but its political purpose transcended this single event in providing the Medici with a permanent triumphalist hall of state. The traditional bellicose themes of marriage coincided neatly with the triumphalist imagery of Vasari's battle scenes: a mock battle was enacted during Giovanna's entry into the city, and actual battles (albeit fictionalized) decorate the Sala Grande to immortalize Florentine triumphs (pp. 179 and following). This section is also accompanied by an invaluable appendix, William S. Anderson's transcriptions and translations of the legends on the city's entry arches and in the Sala Grande.

Here, as in the first section, I wanted an even closer consideration of the inscriptions in relation to their images. Sometimes I missed fuller discussion of the art historical context of the three cycles. Even when complaining, however, the reader is always engaged by the intelligence and multivalence of the authors' argument, the methodological flexibility and sophistication with which Starn and Partridge approach their theme, and the subtle originality of their observations.

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JOHN R. SPENCER. *Andrea del Castagno and His Patrons*. Durham, N.C.: Duke University Press. 1991. Pp. xiii, 179. $34.95.

This book by John R. Spencer probably would not have been written or reviewed in these pages a generation ago before the beginning of regular interchange between historians and art historians studying art in its historical context. Art-in-context studies tend to draw sustenance from the archives and libraries rather than the museums and galleries; they are likely to be more interested in conditions of production and consumption than in ideas, style, or form. The art in art history becomes accordingly a residue to be conventionally appreciated or left to theorists and critics.

Spencer's book is symptomatic of the gains and the losses of this line of work in Italian Renaissance studies. Although he claims to be writing as "an art historian, not as a social historian" (p. xiii), his main focus is not the Florentine artist or his work but Andrea del Castagno's patrons. The legends of the sinister, saturnine painter (ca. 1419–57) are dutifully recounted and suitably debunked; there is little of the routine formal analysis of Castagno's "post-Masaccio" place in the canonical and highly problematic succession of Renaissance styles from Masaccio to Michelangelo. Indeed, art is confined to a stunted catalogue of Castagno's works in the introduction, the mostly conventional stylistic and iconographical readings of individual projects in two long chapters, and the three sets of mediocre reproductions of the book. The archives, however, are very much in evidence as Spencer pursues the labyrinthine network of Florentine patrician ties to locate Castagno's commissions in the web of Medici patrons and clients.

The major beneficiaries of this archival exercise will be the Florentine antiquarians, by birth or by adoption, who can now confidently insert Bernardetto de' Medici, Filippo Carducci, Orlando de' Medici, Girolamo Corboli, and Piero da Gagliano into the jigsaw puzzle of elite genealogies and patrician interests. But neither historians nor art historians will learn much about how patronage worked in Florence that they did not know before from the social histories cited by Spencer. Despite the relentless miniaturization of Florentine studies, Cosimo de' Medici, master *padrino*, remains practically as elusive as ever. And despite (or because of) the fixation on patronage, the interpreta-

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tions of Castagno's pictures as such do not add much beyond a few revisions of detail to previous monographs.

Perhaps the most important critical lesson to be pondered here by those who are out in force to "search for patrons" is the author's own disclaimer: "The lines leading from persons to paintings and back again become a snarled web that is impossible to read" (p. 121).

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CLARE ROBERTSON. *"Il Gran Cardinale": Alessandro Farnese, Patron of the Arts.* New Haven: Yale University Press. 1992. Pp. viii, 323; 212 plates. $45.00.

At the age of fourteen, Alessandro Farnese (1520–89) was made a cardinal by his grandfather, Pope Paul III (r. 1534–49), although his ordination was postponed for thirty years; at fifteen he was promoted to vice chancellor, second only to the pope in the ecclesiastical hierarchy. He accumulated more lucrative benefices than any other cardinal in the sixteenth century, and much of his seemingly limitless wealth was spent on art and architecture. He boasted of possessing the three most beautiful things in the Holy City: the church of the Gesù, for which he selected the architects and donated the entire building costs; the Palazzo Farnese, filled with the family's renowned collection of antiquities; and his daughter. The contradictions inherent in this description reflect not only the complexity of the powerful cardinal, the most important patron of the visual arts in mid-Cinquecento Rome, but also issues at the core of the Protestant Reformation and the Catholic response. Despite the cardinal's importance for the religious, political, and artistic culture of his day, no modern biography exists.

In the past few decades, however, art and architectural historians have initiated detailed investigations of the vast artistic enterprises of the Farnese family. By reevaluating the documents published in the nineteenth century and uncovering new archival evidence, scholars have revised attributions, debated interpretations of iconographic programs, and sought to elucidate the dynamics of patronage. Clare Robertson's impressive and lavishly illustrated book is a significant addition to this burgeoning literature. Her study is the first to offer a comprehensive overview of all the buildings, paintings, sculpture, and decorative arts commissioned over more than fifty years by Cardinal Farnese. She thoughtfully summarizes the conclusions of recent studies of individual monuments and provides extensive new documentation, some of which contributes significantly to tracing the evolution of important projects, such as the early architectural history of the Gesù.

From this meticulous compilation, Robertson observes a dramatic change in Farnese's patronage. The early works were overwhelmingly secular, culminating in the splendid villa at Caprarola (begun in 1556). The great religious commissions were given only in the 1560s, when Tridentine pressures and the cardinal's new personal piety seem to have inaugurated a late religious efflorescence. This constitutes an important addition to our knowledge.

At the same time, the book does not fulfill the promise that the author has led us to anticipate. By asking the reader "to consider what we can expect to learn about Cinquecento artistic development from investigating a patron of this kind" (p. 4), Robertson raises the expectation of a synthetic rethinking of the complex artistic culture of Counter Reformation Rome. Although she rightly refutes some of Federico Zeri's traditionally accepted assumptions about later Cinquecento painting—the "pictorial mysticism" of Giovanni de' Vecchi, the neo-Gothic Farnese culture, the "Farnese style"—she makes little attempt to revise the stereotypical, pejorative stylistic analysis of late Mannerism. The art seems to conform to the author's monochromatic image of the austere Roman Counter Reformation, a view that has been challenged recently by historians.

Robertson's approach toward iconographic interpretation is literal. She posits that "the historical Farnese cycles comprise no more than a straightforward celebration of the family's exploits" (p. 124). Although she may deplore "contrived explanations" of complex allegories at Caprarola (pp. 111, 123–24), the frescoes are not unmediated illustrations of a programmer's *invenzione*. By reductively equating text and image, she disregards without comment all newer critical theory concerning what and how art communicates as well as its contextualized reception. Celebratory works of art such as these are part of a discourse about power. Their visual rhetoric of triumphal spectacle, as recent scholarship by Loren Partridge and Randolph Starn demonstrates, offers new paradigms for exploring the "deep structure" of the authoritarian culture of sixteenth-century Italy. Robertson's unwillingness to cross conventional disciplinary boundaries seems to inhibit a more profound inquiry. The rich fund of information garnered by the author will make this book a standard resource on Farnese patronage, but it will also challenge scholars to delve further into later Cinquecento art by considering the full panoply of visual communication in concert with new artistic practices, patronage patterns, and a nuanced understanding of cultural values.

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MASSIMO FIRPO. *Inquisizione romana e controriforma: Studi sul cardinal Giovanni Morone e il suo processo d'eresia.* Bologna: Mulino. 1992. Pp. 399. L. 40,000.

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In this volume, Massimo Firpo provides a collection of slightly revised essays originally published between 1981 and 1988, all of which provide analysis of the documents he and Dario Marcatto so painstakingly edited in their virtually impeccable edition of the records of the inquisitorial trial of Cardinal Giovanni Morone (1509–80) during the pontificate of Paul IV (1555–59). Here, the title tells all. Firpo uses these studies to make a larger argument that those already familiar with his other writings will immediately recognize: that the trial signals the decisive conflict between rival approaches to institutional reform and the control of heresy at the curial level that ended with the victory of inquisitions and indexes as the primary methods, a victory summed up by the twin elections of Marcellus II (1555) and Paul IV to the papacy. For Firpo, the trial also symbolizes what he calls the maturation of the "break of the Counter-reformation Church with the age of the Renaissance" (p. 355).

Firpo presents Morone and Reginald Pole (1500–58) as the "spiritual" defenders of free thought and discussion in the conflict over control of heresy, and he provides a detailed, important analysis of both prelates in a variety of situations. Morone's work in the diocese of Modena, the rapport of both with the poet Vittoria Colonna (1490–1547), the first phase of Morone's trial, the political ramifications of the trial as well as the defensive phase in which Morone constructed a "counter" image of himself (at times quite falsely) as a determined defender of orthodoxy, all receive attention here, in separate articles. Marcellus II, Paul IV, and Pius V (1566–72) appear as the "intransigent" antagonists, determined to control papal elections, and thereby curial power, by discrediting cardinals like Morone and Pole through inquisitorial investigations. The strength of Firpo's argument lies in analysis of documents that demonstrate Paul's virulent, unreasonable, unjustified, and highly personal vendetta against these two prelates, a program he bequeathed to his protégé, Pius V.

The extension of the argument to suggest that this outlook characterized more than just Paul IV and Pius V is where, in my opinion, a legion of difficulties arise. I have argued elsewhere that linking Marcellus II to these two is misleading, indeed patently false. His earlier career as an inquisitor indicates that he was far more likely to halt unjustified proceedings than to pursue them. The new work on the various indexes of prohibited books from this period indicate just how far out of the mainstream of curial thought Paul IV really was. In fact, evidence provided in Firpo's essays that during their administrations Paul IV and Pius V both attempted (and failed) to subvert standard inquisitorial procedures in order to attack Morone more effectively demonstrates that they resembled each other, but none of their contemporaries in papal and curial administration. And if their policies were truly "triumphant" in the Counter Reformation curia after all, why did neither of them get away with it?

Whether Pius IV's pontificate, or those of Paul IV and Pius V, will ultimately be considered the "parenthesis" in the history of the Tridentine church will depend on the results of a detailed reevaluation of earlier and later popes, but the question does not alter the importance of Firpo's contribution. The book should sell well, and deservedly so, in Italy, but Anglo-American scholars interested in the topic probably encountered these essays in their original form. Some ambitious university press ought to undertake a translation in order to make this view of the Counter Reformation more widely known to English reading audiences.

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ANTHONY M. CUMMINGS. *The Politicized Muse: Music for Medici Festivals, 1512–1537*. (Princeton Essays on the Arts.) Princeton: Princeton University Press. 1992. Pp. xvi, 260. $39.95.

Tales rearranged and reinterpreted in a narrative frame: such is the structure of this impressive retelling of the story of the transformation of Florence from republic to Medici principate as reflected through its "politicized muse," the many festivals that celebrated the consolidation of Medici might between 1512 and 1537. Anthony M. Cummings's thesis is that these festivals, described in histories, chronicles, memoirs, letters, and diaries, were an expression of the changing status of the Medici during the years between the restoration of the family to Florence and the election of Cosimo I as the city's ruler.

The thesis is hardly new, but this synthesis of available material into a readable account that elucidates the context for the works of art generated by these festive events is indeed original. For this reason the subtitle is almost misleading; although the author's principal aim is to reconstruct and classify the music that constituted such an important part of the "acoustical landscape" of these Florentine festivals, he also takes into account all manner of artistic ephemera: triumphal arches, theatrical productions, decorative panels, unpublished poetic verses, fictive sculpture. Compared to eight notated musical examples (only three of which can be demonstrably linked to the occasions described), there are forty-five black-and-white illustrations of works that, although already known to art and theater historians, serve in this context to deepen our understanding of their coded cultural significance. Thus, Cummings makes original and effective use of iconographical and interdisciplinary sources.

In thirteen chapters, each devoted to a specific historical event (such as the election of Leo X, the first Medici pope, or the celebration of John the Baptist's feast day in 1514) or to a constellation of events (such

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as the carnival season of 1513 or the weeks of celebratory activities organized for the wedding of Lorenzo II in 1518), Cummings meticulously recounts every aspect of festivals involving music. Relying always on primary sources, he describes and annotates performances or other happenings as they occurred chronologically in connection with each occasion—*sacre rappresentazioni*, processions, liturgical ceremonies, comedies, and *intermedii*—detailing the extraordinary variety of ways in which music and the other arts served propagandistic and commemorative functions. Everything is profusely documented, although the notes are presented, wisely, in a separate section at the end of the volume. Framing the narrative chapters are an introduction dealing with methodology and a conclusion that is a virtuosic attempt to summarize and classify the types of music accompanying the festivals. There is also a helpful partial genealogy of the Medici family.

Although Cummings avoids technical language in dealing with the musical sources, we learn a great deal about their varied styles and functions. Particularly interesting is his distinction between "static" and "dynamic" elements (pp. 165–66) of the festivals and the inferences he draws about their effect on musical style; verses performed by singers in motion (for example, during a procession) tended to be in a simpler musical style than those sung from atop a stationary arch. Furthermore, such arches frequently bore triumphant inscriptions, visible to audiences as they listened to the singing of a text intended to explicate the inscription; in such cases, the circumstances allowed for more elaborate musical presentation. Cummings also confirms the importance of Lorenzo's musical patronage and his role in the transmission of polyphonic music between Rome and Florence, possibly stimulating the more elevated treatment of Italian texts that led to the emergence of the madrigal. Among many other notable observations summarized in the book's concluding chapter, I am able to cite only one more: the development during this period of the *intermedii*, earlier characterized by stylistic heterogeneity, into the more unified cycle that became a hallmark of the Florentine theatrical tradition.

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Osvaldo Cavallar. *Francesco Guicciardini, giurista: I ricordi degli onorari*. (Per la storia del pensiero giuridico moderno, number 36.) Milan: Giuffrè. 1991. Pp. xxi, 396. L. 42,000.

During the past thirty years, two Guicciardinis have emerged from the literature: the well-known political and historical writer studied by Felix Gilbert, J. R. Hale, J. G. A. Pocock, and Nicolai Rubenstein, among others; and "Guicciardini *minore*," as Osvaldo Cavallar calls him, the hardly known practicing attorney whose jurisprudence was thought to have had little influence on his political thought. A working assumption of Cavallar's book is that our image of both Guicciardinis has become distorted because we have not known enough about Florentine legal practice properly to assess its relationship to the culture's historical and political views. Cavallar's aim, therefore, is to begin the process of balancing and reintegrating political theory and legal practice, first by reexamining Francesco Guicciardini's legal career in the light of unpublished materials and new archival findings, then by exposing the roots of his political theory in the concrete daily particulars of his legal practice. The project will require a second and forthcoming volume on the ideological and political dimensions of Guicciardini's jurisprudence, but this book is an important and substantial beginning.

The present work focuses on Guicciardini's honorarial records, the hitherto unknown *Ricordi degli onorari* discovered in the Biblioteca Nazionale in Florence and published here for the first time as an appendix. The ledger contains 920 entries dating from 1509 to 1520. Each entry indicates the date of payment, the name of the client or institution, and the amount collected. Some entries also mention the type of case in question, and some note as well the service provided. From these records, Cavallar reconstructs a large part of Guicciardini's legal career and situates him in relation to early sixteenth-century Florentine jurisprudence. We learn, for example, that his public commissions involved such issues as jurisdiction, taxation, boundary lines, land use, privileges and immunities, contracts, loans, tenancies, broken truces, and disputes between civilians and the militia. His private clients brought predictably more personal cases dealing with matters like inheritance and patrimony, dowries, and homicide. In these cases Guicciardini might write his own opinions (*consilia*) or subscribe to those written by others; he might provide simple consultation, arbitration, or syndication. Sometimes he only took a matter under advisement.

His clientele ran the gamut of Florentine and Tuscan society, from (not surprisingly) the leading families, wealthy merchants, *contado* nobility, and ranking ecclesiastics to (more surprisingly) small contractors and artisans, including carpenters, bakers, and wool and leather workers. His clients make an impressive and diverse list: the Signory, the Eight of Public Safety, tower officials, the chief commercial tribunal, the court of the bishop of Florence, monastic orders, hospitals, guilds, Jews, women, and doctoral students. In addition we learn about Guicciardini's fee structure, earnings, financial condition, and economic position; his work habits, operating procedures, and case load; his network of contacts, family ties, and professional association with notaries, procurators, and the movers and shakers of the law. And we get a sense of his terminology, writing style, and mind-set.

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In this last context Cavallar begins to show the integration of Guicciardini's legal, historical, and political thought. We see him grappling with concrete issues growing out of Florence's domination of subject cities: how, to use his image, the egg yolk related to the white. There are historical questions of tradition, custom, and past practice, as well as issues of sovereignty, jurisdictional unity, and legal homogeneity. Guicciardini offered reflections on the violent origins of the state and, especially with regard to the loss and recovery of Pisa, justifications for rebellion and reconquest. Especially in the subject territories, his clients forced him to see conflicts over private interests in terms of state and public security as well. What, for example, were a private citizen's rights to confiscated property if the city were in a state of rebellion? It is small wonder that Guicciardini came to develop a profound sense of the forces making for uncertainty in life, or that he came to value caution, discretion, pragmatism, compromise, and flexibility over ideology.

It is no mean accomplishment to show how the law equipped Guicciardini for his ambassadorial and gubernatorial responsibilities, even as it anchored his theorizing in quotidian practicalities. He was all of one piece. His honorarial records and opinions must now be read, as Cavallar has persuasively demonstrated, side by side with his letters, memoirs, political discourses, and historical works. Cavallar's mastery of the bibliography, along with his meticulous attention to detail and caution in interpreting remaining lacunae, make him a most authoritative guide. I look forward to the second volume of this subject.

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OSVALDO RAGGIO. *Faide e parentele: Lo stato genovese visto dalla Fontanabuona.* (Microstorie, number 18.) Turin: Giulio Einaudi. 1990. Pp. xxxi, 260. L. 32,000.

Osvaldo Raggio's book, part of a series devoted to microhistory, is a revealing, detailed study of rural society in the Republic of Genoa in the late sixteenth and early seventeenth centuries. Raggio focuses on the valleys of Fontanabuona, a region along the coast south of the capital just inland from Rapallo, and the relations between this rural world and tentative developments in the Genoese state. It was less fiscal requirements that led the senate to seek firmer control of these valleys than it was the need to establish greater public security. The republic's agents of justice responded to disorder that emanated from a variety of sources: civil divisions within Genoa, independent fiefs and foreign powers, and the rural society of Fontanabuona itself. But the pacification of this region was not achieved in the period of this study, or even before the fall of the Genoese republic at the end of the eighteenth century.

What emerges most clearly from Raggio's expert characterization is the fundamental importance of *parentele*—clan or kinship groups—that mediated Genoese rule at Fontanabuona and affected every aspect of rural life. The senate found brigandage particularly troublesome in Fontanabuona, and Raggio explores it as a key to open the local clans to view. Inheritance, marriage, dowries, landholding, protection offered by the principal houses of a clan to their lesser relations, and the basic need to survive all reinforced clan networks. The forested and agriculturally poor mountainous inland valleys of Fontanabuona were traversed by a continual commerce between the towns of the Genoese coast and those further inland in the Po Valley. Brigandage arose from the inequality of exchanges between these poor valleys and the outside world; the clans offered guides, inns, and either protection or plundering of merchants' mule-trains.

Brigandage also reveals the long-lasting *faide*—vendettas or feuds—among clans that plagued Fontanabuona. The *faide* further indicate the clans' intermediary function, since different sides in local conflicts often borrowed nomenclature from larger political divisions outside of the region: Guelfs versus Ghibellines, Blacks versus Whites, *Nuovi* versus *Vecchi*—these last terms reflect Genoese factional divisions in the sixteenth century. Raggio's careful use of judicial records sheds much light on various aspects of clan organization.

The Genoese state was not effective in acting against *faide*. There was little change in the judicial system in this period, and although the *commissario* installed at Rapallo in 1608 might have acted to break down the solidarity of clans by intervening in disputes within an individual clan or in disputes among clans across regions, the operation of justice remained archaic. *Faide* and clans remained in Fontanabuona in the eighteenth century as a reminder of the incomplete development of Genoa as a modern state.

Raggio expresses the hope that readers will find parallels in the incidents recounted in this book with structures elsewhere in the Mediterranean, and that Fontanabuona might provide a model of relationships between society and the state. The detail of this study is a tribute to the value of microhistory as a technique, but one limitation is that the nature of such parallels or of such a model is not explicitly indicated; what was typical or unique about the relationship between Fontanabuona and the Genoese state is left for the reader to guess. Nonetheless, as a detailed and focused study of the functioning of rural clan networks, this book merits widespread attention among students of the early modern state.

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TOMMASO ASTARITA. *The Continuity of Feudal Power: The Caracciolo di Brienza in Spanish Naples.* (Cambridge

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Studies in Early Modern History.) New York: Cambridge University Press. 1992. Pp. x, 281. $54.95.

Since the 1950s the historiographical debate over the Mezzogiorno in the early modern period has been cast in terms defined largely by Rosario Villari. In his rich analyses of the economic and political history of the kingdom of Naples, Villari argued that a process of refeudalization in the first half of the seventeenth century was the major cause of the revolt of Masaniello in 1647–48 and that the consequent cooperation of the Neapolitan aristocracy with their Spanish (and later their Austrian and then their Bourbon) rulers blocked any real possibility for economic development in the *ancien régime*.

Among Italian historians, this debate (with Giuseppe Galasso emerging as Villari's leading critic) has spawned numerous studies, but it is only in recent years—thanks above all to the excellent contributions in English by Antonio Calabria, John Marino, and now Tommaso Astarita—that British and American readers have had an opportunity to explore the history of the Italian south in less polemical and more nuanced terms than those with which they have been familiar.

The Caracciolo was a large and distinguished aristocratic clan that included at one time as many as fifty families (or titled lines). One of these families—the Caracciolo Brienza and its history from the sixteenth to the early eighteenth century, the period of Spanish rule—is the subject of Astarita's study. In the mid-sixteenth century, this family acquired the fiefs of Brienza, Pietrafesa, Atena, and Sasso, which, together, served as the center of its feudal patrimony. This *stato*, although located in the isolated and poor countryside to the southeast of Eboli, made the family among the hundred wealthiest of the realm. In 1569 the family also acquired the title of marquis. And while family members occasionally resided in Naples (to conduct business or to arrange marriages), the Caracciolo Brienza appear to have preferred residence in their fiefs. Perhaps they did so, because their status there was clearly dominant. As Astarita observes, the "physically prominent position of the castle, and the fact that it was at once the baron's residence, the economic center of the feudal domain, the judicial center of the village, and the jail, made it the most visible symbol of feudal rule" (p. 122).

But this book is much more than a case study of a baronial dynasty. Through a meticulous examination of a vast range of archival sources, Astarita makes an important contribution to our understanding of what he has rightly called "the continuity of feudal power." He does so first by developing his analysis of Neapolitan society on its own terms. In doing so, he avoids the somewhat tired question of why the kingdom of Naples "failed" to develop in accordance with models that apply more readily to the north of Italy or to England. Specifically, this approach has made possible the development of a careful analysis of the economic behavior of the Caracciolo Brienza over a period of nearly two centuries in which we find the family adapting intelligently to the economic, social, and political changes around them. They were cautious managers who emphasized the conservation of stable revenues over economic innovations; they remained paternalistic in their relations with their vassals and the communities subject to them; they maintained a strong clan identity in their pursuit of marriage policies; and they even benefited from the growth of Spanish government—especially from a judicial system that they turned to their advantage.

This book should interest all scholars concerned with the history of the early modern world, but it is essential reading for students of Italian history. Astarita's detailed portrait of the traditional world of the early modern Neapolitan aristocracy demonstrates just how fragile earlier constructs of the history of this class have been. Moreover, Astarita does much to clarify the nature of aristocratic values that would reassert themselves throughout Italy in the sixteenth and seventeenth centuries. Finally, by drawing on the research of scholars into the history of aristocratic families throughout the peninsula, Astarita's study breaks out of the *campanilismo*, or regionalism, that continues to limit much of the research into the history of late-medieval and early modern Italy.

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BARBARA EVANS CLEMENTS *et al.*, editors. *Russia's Women: Accommodation, Resistance, Transformation*. Berkeley and Los Angeles: University of California Press. 1991. Pp. xi, 300. Cloth $45.00, paper $14.95.

This collection brings together some of the best recent scholarship of social historians on Russian women. The book is divided into two uneven chronological parts: the first covers the Middle Ages to the decades after the Emancipation, and the second covers the period since 1861, when economic, political, and social changes transformed traditional patriarchy. It is thus a corrective to the prevalent periodization of Russian history into pre and post–1917 periods.

The articles in the first half of the volume depict women's "accommodation and resistance" to traditional patriarchy, a system that emerges as quite complex, allowing some women a measure of autonomy. In her introduction to this half, Christine Worobec shows how women's economic contribution blurred the boundaries between the private and public realm. N. L. Pushkareva's article on women and the medieval family, the single contribution by a Russian scholar, focuses on the blending of pre-Christian and Christian traditions of betrothals and marriage, their canonization by the Orthodox church, the introduction of bridal virginity and of

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celibacy as Christian virtues, the proscription of contraception, abortion, and infanticide by the church, and the growing double standard in the church's efforts to control behavior. Nonetheless the patriarchal system gave women the right to divorce adulterous husbands or those who forced their wives to have sex. Similarly, according to Nancy Shields Kollmann, the Law Code of 1649 gave women the right to protect their honor and to claim compensation for rape and dishonorable conduct. Women's honor was given greater value, to a large extent to protect the very nature of patriarchy.

Eve Levin examines the sixteenth and seventeenth-century notion of impurity surrounding childbirth, which secluded mothers and newborns for forty days following birth and which possibly spared them from potentially harmful contact and debilitating household labor. A different picture emerges from David L. Ransel's article on mortality and infant care culture in the nineteenth century. Traditional seclusion served the psychic need to protect mother and baby from evil spirits and intentions rather than from pathogens. In fact, Russian infant care culture, which included unsanitary care during labor and birth and poor nutrition (especially the early introduction of contaminated solid food and the devaluation of breast feeding), contributed to infant mortality rates of well over half of all babies born. This contrasts sharply with the Muslim populations in those provinces, whose infant care culture allowed for much lower infant mortality rates, although at a price to childbearing Muslim women, who had a higher mortality rate. Ransel raises the specter of prioritizing done by most societies, which assumes stark dimensions in poorer ones. In the Russian village, women were deemed more valuable than their babies because their labor was essential to survival. Like poor mothers in some present-day developing countries, Russian women accepted the fact that many of their children would not survive infancy.

Like Ransel, Valerie Kivelson places her study of witchcraft and witch hunts in comparative perspective. In contradistinction to the West, most victims of Russian witch hunts were men. Kivelson raises the possibility that the strengthening of serfdom might have made Russian women less marginal and more tied to and protected by family networks than the Western European and North American victims of the hysteria. Rodney Bohac's rich study of widows in nineteenth-century serf communities could have profited from a comparison with the early modern West, where widows also had a measure of economic independence in relation to their single and married sisters.

The second part of the collection is pulled together by an incisive introduction by Barbara Engel, in which she underscores the role of women as simultaneous transformers and preservers of tradition. Rose Glickman brings our attention to the enduring peasant healer. Typically a female occupation, healers served as revered midwives, herb curers, abortionists, and exorcists and, despite their impoverishment, led an autonomous and respectable existence. Women's participation in cottage industry was just as important as the healer to peasant survival. Judith Pallot analyzes women's employment by Moscow merchants who supplied them with raw materials and marketed the product, and by factories, which contracted them to complete a distinct phase of the production process. These were the most exploited workers, lacking even the inadequate protection that factory workers enjoyed under the law. Unlike the healers, the factory workers had a paid occupation, which, although vital to the peasant household, did not earn them concomitant status or autonomy.

The editorial decision not to use 1917 as a divide worked best for the two articles on abortion. Laura Engelstein looks at the debate over abortion by the medical and legal professions in the years immediately before World War I. Although the participants in the debate were progressive by any standard, the discussion betrayed the anxieties of middle-class men as they viewed urban women of their own class and women workers exerting control over reproduction and sexuality. Only a small minority of the debaters championed women's rights to control their bodies, even though most supported a relaxation of the abortion law. Abortion was legalized in 1920 as a medical and legal expediency, not on the basis of women's right to determine fertility. According to Wendy Goldman, in the 1920s and 1930s abortion became the most prevalent birth control method among urban women. By the 1930s pro-natalists decried the fact that there were twice as many abortions as live births and stressed that most women seeking to terminate pregnancies were not the most destitute. Instead, young married women were expressing their preference for smaller families and their assumption of control over their bodies and their lives. This trend, together with the ravages of forced collectivization and massive migration into industrial centers, culminated in the sharp drop in the birth rate, to which the state reacted by criminalizing abortion in 1936.

The widespread use of abortion reflected the transformation in women's aspirations and life goals brought about by the revolution of 1917 and, as Alfred Meyer suggests, by World War I. Despite the stresses caused by the mobilization of fifteen million men, the displacement of millions of civilians, the feminization of agriculture, and the general restructuring of the economy, the war opened economic opportunities to women and revamped gender roles and gender consciousness. Meyer closes on an intriguing note: women's experiences, as expressed in letters to the front, strongly affected the soldiers' morale, with fateful consequences for the war effort and the tsarist state.

Neither Meyer nor the authors of subsequent articles elaborates on women's political role, an obvious

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gap in this otherwise admirable collection. Women and politics are only covered in Elizabeth Waters's essay on the female figure in political posters and monuments. An article on women's influence on the political process in 1917 and thereafter, or on their role in the Civil War or the countryside in the 1930s, to name two examples, would have gone a long way in filling the gap between Waters's innovative article and Barbara Evans Clements's excellent survey of Russian women since 1930, which serve as a conclusion to the volume. This does not detract from a collection that will prove invaluable to scholars and undergraduates alike.

ISABEL A. TIRADO
William Paterson College

PAUL R. JOSEPHSON. *Physics and Politics in Revolutionary Russia.* (California Studies in the History of Science, number 9; Studies of the Harriman Institute, Columbia University.) Berkeley and Los Angeles: University of California Press. 1991. Pp. xix, 423. $39.95.

Paul R. Josephson offers a detailed examination of the development of the discipline of physics in Russia from the late imperial period until the death of Joseph Stalin. Whereas other scholars have analyzed events in Soviet physics as part of broader investigations of Soviet science, Josephson's monograph is the first, based on both archival analysis and extensive interviews, that is devoted entirely to this important field of science. Focusing on events and personalities in St. Petersburg (until after World War II indisputably the center of Soviet physics), Josephson chronicles the growth of institutions for training and research in physics, the emergence of a strong and cohesive community of physicists, the research strengths of that community, and its interactions with the larger society and with the international scientific community. A central focus of the book, as indicated by its title, is an analysis of the dynamic relationship between the physics community and the state.

The era covered by this work was marked by dramatic and controversial changes both in theoretical physics and in the relationships between physicists and the state in many countries. Developments in quantum mechanics and relativity theory prompted philosophical debates throughout the Western world at the same time that the rising cost of doing physics, in combination with its potential strategic significance, altered the character of physicist-government interactions. As Josephson demonstrates, Soviet physicists were compelled to deal with these same issues; for this community of scientists, however, the problems they represented were particularly complex.

Josephson chronicles the effects of the revolutions of 1917 and the ensuing Civil War on physics. (In some respects the difficulties of those years are painfully reminiscent of the problems with which scientists in the former Soviet Union are now confronted.) He also presents convincing evidence that physicists were able during the 1920s both to expand significantly the size and scope of their activities and, despite growing administrative restrictions, to resist most ideological pressures. The administrative complexities of that era are clear from Josephson's account. Merely keeping track of all the acronyms proves daunting at times, even for readers familiar with the intricacies of Soviet bureaucratic institutions of that period. Fortunately, Josephson provides the reader with a helpful glossary.

The greatest challenges to the physics profession occurred during the Stalin era, and it is to this period that Josephson devotes the greater part of his attention. Stalinism, of course, had profound effects on many fields of science, the best known example of which is the near destruction of Soviet genetics during the lengthy period when it was dominated by T. D. Lysenko. Although Soviet physicists (and the field of physics as a whole) suffered during this era, the devastation was not as extensive. One of Josephson's primary aims is clearly to account for the somewhat different fate of physics under Stalin.

While taking note of the significance of the strong scientific tradition that had been established by Soviet physicists, Josephson places more emphasis on the important role played by individual leaders of the profession (prominent among them A. F. Ioffe). The unwillingness of these courageous individuals to compromise their science in the name of ideology and their readiness to take their concerns to the top leadership of the country helped both to rescue some of their colleagues and to protect their discipline from extensive damage. (The fact that members of the profession were engaged during those years in developing the bomb also apparently helped to protect the field.) As Josephson demonstrates, not all of these individuals emerged from that difficult era unscathed. Even Ioffe, who was for decades the commanding presence in Leningrad physics, eventually succumbed somewhat to the intense pressures for ideological conformity. Nonetheless, in the final years before Stalin's death he too was relieved of his position, as both ideological pressures and anti-Semitism reached new heights.

This is a thoroughly researched account of the response of an important scientific community to extraordinary external political pressures. As such it should be of great interest to specialists both in Soviet history and in the history of twentieth-century science.

JULIE V. BROWN
University of North Carolina, Greensboro

HUGH D. PHILLIPS. *Between the Revolution and the West: A Political Biography of Maxim M. Litvinov.* Boulder, Colo.: Westview. 1992. Pp. xii, 244. $29.95.

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In this work, Hugh D. Phillips tackles the daunting task of examining the life of one of the most important Bolshevik bureaucrats. Giving only brief mention to Maxim M. Litvinov's early life and his initial involvement with the revolutionary movement in Russia, the author quickly moves on to an examination of the height of Litvinov's career as Soviet plenipotentiary. Paying particular attention to Litvinov's role in the plethora of international conferences of the 1920s, Phillips details Litvinov's promotion of a policy of disarmament and peaceful coexistence and his preference for dealing with Great Britain over Germany, a policy that put him at odds with the commissar for foreign affairs, Georgi V. Chicherin. Litvinov, Phillips argues, was not typical of the Bolshevik bureaucrats that haunted the Kremlin but rather was the "personification of a businesslike diplomat" who was "genuinely, if grudgingly, respected in the west" (p. 177). This characteristic gave Litvinov considerable flexibility in his ability to negotiate with the West throughout the dangerous period of Stalin's rise to and consolidation of power.

There are a number of weaknesses in Phillips's biography of such an important personage in Soviet history. The first is the absence of any Soviet archival sources, in particular Litvinov's personal papers. Phillips explains that he was twice denied access to the Soviet Foreign Ministry Archive. The reader can only deduce, therefore, that the "previously unavailable archives" referred to on the dust jacket are the interviews with Litvinov's surviving relatives and acquaintances, a fact that may explain the rather stilted impressions of the diplomat's life. Phillips's biography is indeed laced with a number of dubious extrapolations that are based largely on interviews and published sources. According to Phillips, Litvinov quickly became disenchanted with party politics and did not demure in disagreeing with his superiors in Moscow. He is described as having been critical of the excesses of Bolshevism and of having openly rejected Stalinism and survived. Although Litvinov may have indeed expressed such opinions privately, it is dubious that such a sentiment would have ever been expressed publicly. Recent archival revelations show the Soviet regime of the 1920s and 1930s as even less forgiving and tolerant than the most grim of the previous Western assessments. Under such circumstances, Litvinov's chances for survival were slim to begin with. Although Phillips does attempt to explain this aberration, he does not come to any satisfactory conclusion as to why Litvinov's life was spared while others far less prominent lost theirs.

There are a number of other unsettling aspects of Phillips's assessment and analysis of Litvinov and the circumstances in which he served as Soviet plenipotentiary. Although he has obviously combed through British Foreign Office and Cabinet records, Phillips gives no indication of understanding political conditions in Britain. Examining the failure of Litvinov's initial appointment to Great Britain, Phillips makes the incredible speculation that "perhaps the British could have avoided some of the unfortunate events by simply granting diplomatic recognition to Litvinov" (p. 28). He also gives an amazing degree of credit to Litvinov for having succeeded in "changing completely the direction of Soviet foreign policy" (p. 137).

That Litvinov played a critical role in shaping Soviet foreign policy and, indeed, Soviet history is indisputable, as this brief biography shows. Used with care, this work provides a valuable—if one-sided—glimpse of Litvinov the man and his times.

CHRISTINE A. WHITE
Pennsylvania State University

JONATHAN HASLAM. *The Soviet Union and the Threat from the East, 1933–41: Moscow, Tokyo, and the Prelude to the Pacific War.* (Pitt Series in Russian and East European Studies.) Pittsburgh, Pa.: University of Pittsburgh Press. 1992. Pp. vii, 208. $49.95.

Jonathan Haslam's volume on Soviet foreign policy in the 1930s is an excellent, balanced, and well-written contribution to the literature. It is valuable both for specialists in the area and for those with a general interest in Soviet foreign policy during the critical decades of the 1930s. Soviet-Japanese relations in the 1930s, hitherto a relatively neglected topic, receives its proper attention here. Given the significance of the Japanese decision to attack south and not north in 1941, it is surprising we have waited this long for such a volume. But the wait has been worthwhile.

Haslam chronicles the changing Soviet-Japanese relations, from a weak Soviet Union appeasing a stronger Japan in the early 1930s to Soviet victories at Lake Khasan and Khalkin-Gol in 1938 to Japanese appeasement of the Soviet Union in 1939–41. Haslam also considers the troubled relationship between the Chinese Communist Party (CCP) and the Comintern in the 1930s as well as the place of Indochina in the 1930s.

This volume has a number of strengths. Its topic, hitherto neglected, is important to understanding the survival of the Soviet Union during a critical period. Haslam reveals the complexity and sophistication of Soviet policy, which, despite isolation, achieved victories in the east that it was denied in the west. He stresses the non-monolithic nature of Soviet foreign policy decision making, which was marked by numerous political and personal conflicts involving the party elite (Stalin, Zhdanov, and Molotov), diplomatic elite (Litvinov and Karakhan), and military elite (Tukhachevskii and Bliukher). Haslam's coverage of the Soviet-Japanese border clashes in 1938 and 1939 is particularly graphic. The important role of Soviet spy Richard Sorge in Tokyo is well documented. The continuing salient role of Moscow over the CCP is nicely etched. He provides a series of useful biographies of all the key actors in the period. Haslam also

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presents exhaustive documentation of all key events in the period.

There are, of course, problems. In the post-Soviet world one wonders what is in those Soviet archives formerly closed to Haslam. We yearn for further examination of the interaction between the traditional diplomacy, rooted in the debacle of 1904, and revolutionary diplomacy. We also wish for a little more consistency in the treatment of Stalin and wonder if the condemnation of Stalin for his "brutal response to external threats" (p. 164) in 1938 and 1939 is not too easy. Would the Japanese militarists, who had just overrun Nanjing and would soon launch a surprise assault on Pearl Harbor, have understood any other language?

But these are quibbles with an author who has done extremely well with very limited material. This is a short volume deserving a large audience, even more so in the post–Cold War era of the 1990s where regional conflicts of the Soviet-Japanese kind threaten to upset global stability now that the old superpower-imposed order is gone.

JONATHAN ADELMAN
University of Denver

NEAR EAST

WALTER F. WEIKER. *Ottomans, Turks and the Jewish Polity: A History of the Jews of Turkey.* (The Milken Library of Jewish Public Affairs.) Lanham, Md.: University Press of America or the Jerusalem Center for Public Affairs, Jerusalem. 1992. Pp. xvi, 369. $47.50.

Walter F. Weiker attempts to cover the history of the Jews in Asia Minor and the Balkans from roughly the sixth century B.C. to present-day Turkey. The book's publication coincided with the 500th anniversary of the expulsion of the Jews from Spain and their settlement in the Ottoman empire with the express welcoming order of Sultan Bayezid II. The book provides a sweeping panorama, dotted with statistics, tables, but not a glossary, of nearly two thousand years of Jewish history, although two-thirds of the book deals with the modern period. Different from half a dozen books on Turkey's Jews published during the last five years, Weiker attempts to provide, in addition to historical and cultural information, a quantitative economic and social profile of the Jewish community. Moreover, he supplies welcome information on the immediate past and contemporary situation of the Turkish Jews, including their migration to and life in Israel (Weiker has studied in a different work the life of Israeli immigrants from Turkey. See his *The Unseen Israelis: The Jews from Turkey in Israel* [1988]). The general impression one is left with after reading the book is that Jews felt safe and had a rather good life in Turkey. That two-thirds of Turkey's 120,000 Jews (in 1946) left the country for Israel was due, as Weiker puts it, to economic reasons: most emigrants were poor, while the well-to-do seemed to blend to the point of assimilation into the huge Turkish majority.

It is easy to dispute many of Weiker's points, because he tries to cover too much and too quickly, and he tends to devote too much space to trivia while treating lightly the basic points. For instance, he deals with the coming of the Spanish Jews to Turkey as just another phase in the long history of the Jews. In reality, that emigration was a qualitative, cultural, and political threshold that separated the medieval from the modern Jew. The Sephardic immigration to the Middle East was, in a way, the first "aliyah," which carried with it the learning, sophistication, and wealth of the Sephardim, but it was also the painful cultural and spiritual experience of many *conversos*, who acted as Christians during the day and as Jews at night, perhaps internalizing the values of both religions. One can say further that the Muslim *tarikats* (popular religious Sufi orders) in the Balkans were a model for popularizing Jewish *Kabbalah*.

Weiker devotes a few perfunctory pages to Sabatai Sevi (Zvi), based almost entirely on Gershon Scholem—not on his classical work but on a summary article that appeared in *Encyclopaedia Judaica* (14 [1986], 1219–54), and consequently he fails to evaluate properly the profound and hitherto ignored or deprecated impact of Sevi's messianic experience in the sociopolitical evolution of Jewish society. One is overwhelmed by Weiker's monotonous listing of facts and figures but relieved by an occasional interesting quotation from some old source. One has the impression that the author, probably anxious to publish the book for the 500th anniversary, did not have much time to use selectively his sources and digest all the material. The result is a quite readable and useful but undistinguished book.

KEMAL H. KARPAT
University of Wisconsin, Madison

PETER THORAU. *The Lion of Egypt: Sultan Baybars I and the Near East in the Thirteenth Century.* Translated by P. M. HOLT. New York: Longman. 1992. Pp. xiii, 321.

DOUGLAS PATTON. *Badr al-Dīn Lu'lu': Atabeg of Mosul, 1211–1259.* (Occasional Papers, number 3.) Seattle: Middle East Center, Jackson School of International Studies, University of Washington; distributed by University of Washington Press. 1991. Pp. viii, 111. $12.95.

As a genre, political biographies of powerful men may have gone out of fashion in many contemporary historical circles, but they are still welcome additions to the relatively underdeveloped historiographical literature about the premodern Middle East. The books reviewed here enhance our understanding of that region during the turbulent period of the Crusades and the Mongol invasions by examining the lives of two nearly contemporary rulers, Badr al-Dīn

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Lu'lu', effective ruler of Mosul and its environs for four decades down to his death in 1259, and Baybars I, sultan of Egypt and Syria from 1260 to 1277.

Douglas Patton's brief but detailed study consists of three chapters: one on the intricate political struggles in northern Mesopotamia as the strength of the Zangid dynasty waned and in the course of which Lu'lu' progressed from being a talented slave-soldier amir to regent and then to independent ruler; another on Lu'lu' as ruler, which examines his relations with the Abbasid caliphate and other regional dynasties, the limited expansion of the territory under his control, and his collaboration with Hülegü and the Mongols; and a final chapter on his short-lived dynasty and the generally unhappy fate of his descendants. Although Patton's account may be short, it is by no means uncomplicated. He has done a commendable job of bringing together the available historical material, much of it still in manuscript sources, and constructing a comprehensive and reliable account of the political history of the Mosul area during this period. Patton attempts to find some broader historical significance in his subject by suggesting that Lu'lu"s diplomatic and military maneuvering contributed indirectly to the decline of the Ayyubid dynasty and the Mongol destruction of the Abbasid caliphate. This perhaps gives Lu'lu' more credit than he deserves: both events would probably have occurred without him, and in any case he was motivated by the short-term political expediency of bolstering his own rule and hardly envisaged or even wanted either result. Patton is on firmer ground in noting Lu'lu"s reputation as a stern but just ruler and his patronage of the arts, but unfortunately he does little or nothing to explore these facets of Lu'lu"s rule or to flesh out his personality. Instead, Lu'lu' emerges from this study primarily as what today would be described as a "survivor"—a ruler who is shrewd, calculating, realistic, and somewhat unprincipled, but who can maintain power and order in an environment which is at best precarious. In that sense, this book represents a well-researched and carefully written, but rather one-dimensional, political biography of a petty dynasty in an obscure corner of the medieval world.

Although Baybars reigned for a shorter period of time, he was clearly a much more important historical figure than Lu'lu', and there is a richer array of source material for reconstructing his life and times. Peter Thorau has taken full advantage of this to produce a masterly account of Baybars and the Levant in the thirteenth century. With remarkable clarity and insight, Thorau traces the collapse of the Ayyubid dynasty; the factional struggles among the slave-soldiers (Mamluks) of the old regime; the steady, willful rise of Baybars to supreme power among the commanders of the Mamluks; and the strategy and implementation of Baybars's foreign and domestic policies. He is equally skillful in bringing Baybars to life as a rather ruthless and sometimes superstitious but energetic and effective ruler. Throughout, Thorau's control of the complex Arabic, Persian, Greek, Latin, and Armenian source material is sound and impressive, as is the analysis of and comments on the secondary literature. One might argue that Thorau has merely refined earlier interpretations of Baybars and does not offer a radically new or revisionist assessment. He certainly continues the tradition of being almost adulatory in evaluating Baybars: he suggests that Baybars may have exemplified a putative Mamluk ideal of the *Heerkönig*; argues that he used only as much violence as necessary to attain his objectives; notes his great strategic, diplomatic, and military finesse in limiting Mongol expansion and eliminating the crusader states; and gives him credit for stabilizing the government of Egypt and Syria through his clever manipulation of the bureaucracy, religious leadership, and the institution of the "shadow caliphate." This idealization of Baybars as a ruler is somewhat jarring since it is in stark contrast with Thorau's rather unflattering account of Baybars's early career and usurpation of power and his perhaps overly harsh assessment of the depiction of Baybars by his official biographer, Ibn 'Abd al-Zahir. Nonetheless, Thorau's book represents the definitive biography of Baybars and is likely to remain so for quite some time.

Elton L. Daniel
University of Hawaii, Manoa

Jonathan Berkey. *The Transmission of Knowledge in Medieval Cairo: A Social History of Islamic Education.* (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1992. Pp. x, 238. $39.50.

Notwithstanding the self-perception of medieval scholars as guardians of an organic body of "legitimate" knowledge, Jonathan Berkey recognizes that individual teachers transmitted to their students "less an abstract body of knowledge embodied in particular texts than a personal authority over those texts" (p. 34). His main concern is to examine "the people, processes, and institutions involved in higher education in Cairo from the middle of the thirteenth to the beginning of the sixteenth centuries" (p. 12).

Berkey explores the essential tension between the "informal" system of instruction and personal relationships, which he acknowledges as central to transmitting Islamic religious knowledge, and its relationship to the *madrasas*, or schools of higher learning, funded by pious endowments (*waqfiyyas*) bestowed by the powerful. The literal meaning of *madrasa* is "a place of learning," and Berkey acknowledges that since education was "grounded in the personalities of individual teachers," it took place wherever a professor taught. Some spaces for learning were considered more sacred and important than others, but in the end learning was legitimized by the reputations of individual scholars and not by the formal schools with

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which they were associated or the endowments from which they benefited. Much learning took place outside of formal, endowed religious establishments and the support of a wealthy patron was no guarantor of scholarly excellence, but the allotment of stipends, accommodations, and places to study and teach sometimes led to fierce competition among teachers and their disciples.

Berkey characterizes higher religious education in late-medieval Cairo as "utterly dependent on the rambunctious, mostly Turkish-speaking, dubiously Muslim Mamluk soldiers" (p. 128). The reasons for Mamluk support for higher education varied over time. At first it was to combat Shi'i influences. Later, endowing, and sometimes controlling appointments to, a prominent institution of learning conferred on Mamluk patrons "a potent symbol of symbolic legitimacy" (p. 131). Carefully managed, the excess funds for a *madrasa* endowment could also protect personal wealth and indirectly insure its transmission to descendants (p. 134). Although women played "virtually no role" in formal education, some were educated informally at home, established schools, and in old age became prominent transmitters of *hadith*, the sayings of the prophet. Women were systematically excluded from the legal, religious, and bureaucratic occupations for which religious education prepared candidates, but they nonetheless could be influential as donors and administrators (pp. 164–66).

Berkey's argument is best when tracking the linkages between scholars and their wealthy Mamluk patrons and the often intense rivalries over securing patronage and controlling *madrasas*, *waqfiyya* funds, and teaching appointments. Senior scholars sought to control both stipends and appointments, and these did not necessarily go to the best and the brightest. Scholars commonly sought to promote members of their own family (pp. 119–21), and appointments to teaching, legal, and religious posts were routinely bought (pp. 97–98). Even when Berkey's sources, including *waqfiyya* documents and biographical dictionaries, do not allow him fully to trace rivalries over appointments and patronage, the sociological questions that he poses serve as a useful guide to scholars interested in higher Islamic education elsewhere in the Muslim world.

One larger issue overshadows Berkey's excellent monograph. Berkey acknowledges that much religious learning took place outside the formal institutional framework of *madrasa* and was "tied to persons rather than [formal] institutions" (p. 16, 46, 56, 217), and therefore is not fully convincing when he asserts that "by the middle of the thirteenth century the madrasa had established itself as the primary forum for religious education," especially in Egypt and Syria (p. 9). Because such personal ties are "informal" they are no less institutional—the basic sociological meaning of "institution," an accepted way of accomplishing a social action, is not tied to the narrower sense of formal corporate or organizational activities. Nonetheless, this book goes beyond George Makdisi's pioneering work in making sense of how medieval *madrasas* worked and offers major insight into the social and political context of religious education in the Muslim world.

DALE F. EICKELMAN
Dartmouth College

FARZANEH MILANI. *Veils and Words: The Emerging Voices of Iranian Women Writers*. (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1992. Pp. xix, 295. Cloth $34.95, paper $16.95.

Farzaneh Milani has written an original book about Iranian women's literature with a distinctly literary prose of its own that makes it pleasurable reading while pursuing an important argument. Although in recent years a number of short stories and novels by Iranian women have been published in English translation, this is the first book about that body of literature, thus making it a pathbreaking enterprise and securing it as a point of departure for further critical work.

Although the book covers Iranian women's writings, many of its arguments, particularly as they pertain to women's voices and bodies in public, are relevant to women's writings elsewhere in the Islamic Middle East, where there are culturally shared notions of women's proper place and language. Milani's book also relates to a growing field of critical studies of women's writings from non–Euro-American cultures and languages. Milani draws skillfully on her mastery of Persian literature as well as her familiarity with literary theory and women's cultural studies.

Milani proposes a strong argument: a connection between women's voice, their literary self-revelation, and unveiling—in other words, their bodily self-revelation. It is this central idea that holds the book together. Moreover, Milani pursues an argument without proclaiming a cause; her main thesis—that "the norms and values that regulated women's physical concealment applied equally to their literary expression . . . a woman's physical invisibility was completed by her silence, her public nonexistence" (p. 46)—is investigated historically and textually. It is a text of persuasive interpretation, of interpretive persuasion.

The first chapter introduces the reader to the literature and the author, herself a cultural migrant. Her use of autobiographical observations is successful in focusing one's attention immediately on the theme of the book, the transgressive character of Iranian women's literary endeavors in their particular historical and cultural context. This theme of transgression is then pursued in its historical context, correlated to the modern history of unveiling—and reveiling—from the mid-nineteenth century to the present.

Milani next focuses on particular individuals. What

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is most rewarding here is the combination of keeping to the particular significance of these individual writers and poets while not losing the thread of Milani's thesis. She pursues her own line of argument without calling on previous writers and poets as simple witnesses to prove her case before the jury of readers. Chapter 4 extends the author's argument about the connection of woman's voice and body to Qurrat al-'Ayn's death, her killing and the silencing of her voice. The three subsequent chapters focus on three important and very different women poets, Parvin I'tisami, Furugh Farrukhzad, and Tahirah Saffarzadah. In each chapter the author investigates the particular poet's style, content, and form of poetry, indicating the interconnections. The next two chapters deal with women's fiction and autobiographies. A final chapter discusses the significance of the proliferation of postrevolutionary Iranian women's writings. Drawing on the recent poetry of Simin Bihbahani, Milani argues that the new women's voices, "through the veil," are breaking down the competition between discourses of "modernity" and "tradition," both Iranian and Western, forming a new synthesis that "proves the instability upon which the dialectic between the two traditions is played" (p. 234).

AFSANEH NAJMABADI
Barnard College

RACHEL SIMON. *Change within Tradition among Jewish Women in Libya.* (A Samuel and Althea Stroum Book.) Seattle: University of Washington Press. 1992. Pp. vii, 221. $30.00.

The study of Jewish communities in the modern Middle East has only in recent years begun to receive scholarly attention, although Libyan Jewry has been the focus of a number of historical and anthropological studies, most notably by Harvey Goldberg, Renzo De Felice, and Rachel Simon. Simon's book is the first historical study devoted to Jewish women for any Middle Eastern country in the modern period and one of only a few scholarly works on the history of Middle Eastern women.

Written sources on women in the Middle East are relatively few and are generated mostly by men. Coupled with the general paucity of material on women is the rather limited access to historical material for Libya. Nonetheless, Simon has done an excellent job working with the available material, which is sometimes limited to just a few sources. Frequently cited is a manuscript written by a Libyan Jew at the turn of the century, Mordecai Ha-Cohen's *Higgid Mordecai*, which Harvey Goldberg edited and analyzed. In the late nineteenth century, the Alliance Israélite Universelle began establishing Jewish schools in Libya, and Simon makes thorough use of the archives of this Franco-Jewish organization for the period prior to World War I. The most important source used for the following decades until the 1950s, when most Libyan Jews emigrated, are the Central Zionist Archives. Almost entirely missing are women's voices, which could only have been recovered through oral histories of Jewish women from Libya now mostly residing in Israel.

The sources used have defined and circumscribed the kinds of questions asked. The first chapter looks at women's status in the family and in public life. Here the author investigates customs in the home, participation in ceremonies, and the code of modesty characteristic of Muslim society. This type of ethnographic reconstruction (mainly looking at the Ottoman period and mostly relying on *Higgid Mordecai*) is continued in the second chapter on family life, which focuses on marriage customs and relations with Muslims. The following chapters devote more attention to change brought about by the external influence of the Alliance Israélite Universelle, Italian colonialism, and Zionism. In a chapter on work, Simon shows how women found new types of employment outside the home, especially in teaching. The avenue for this transition was through modern education, the subject of the following chapter. Added to the French and Italian schools were Hebrew schools, first established during the British occupation of Libya in World War II, the result of the presence of a significant number of Jewish soldiers from Palestine. Women who were educated were therefore inculcated in the modern Western and Zionist educational systems, since more traditional Jewish education remained the exclusive preserve of men. The rather limited participation of women in public life gradually began to change in the late nineteenth century and the twentieth century because of modern education, new vocational opportunities, and, above all, the Zionist movement, which demanded the participation of women in its activities. Changes in the status of Jewish women up until the final dissolution of Libyan Jewry in 1967 remained somewhat restricted to certain elements in the higher socioeconomic strata and were accepted by the male-dominated society to the degree that such changes were necessary for the survival of the community and maintenance of its cultural identity.

Simon's study could have benefited from more comparative analysis to help situate the Libyan case in the history of both Jewish women and Middle Eastern women. Libyan Jewish culture was influenced by Muslim society and by Jewish traditions, which Simon sometimes attempts to disentangle although often it is far from clear what constitutes Muslim as opposed to Jewish influence. Popular cultural practices are often associated with Muslim society and juxtaposed with formal Judaism defined by Jewish law. This analysis does not offer an alternative women's history because it accepts the male version of what constitutes Jewish culture based on the patriarchal system of rabbinical law, which relegates women to the domestic sphere on the one hand and the realm of nature (rather than "culture") on the other. Excluded from many of the

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formal male cultural activities, it could be argued that women often developed their own "Jewish" rituals and responded in ways significantly different from men to the forces of social and political change that dramatically affected Libyan Jewish society in the twentieth century. At the same time, as the first major study that investigates the institutional and political changes that affected the lives of women in a Middle Eastern Jewish community, Simon invites interesting comparisons and helps point us in the direction of understanding the lives of Jewish women in the modern history of the Middle East.

DANIEL J. SCHROETER
University of Florida

AMIRA EL AZHARY SONBOL. *The Creation of a Medical Profession in Egypt, 1800–1922*. (Contemporary Issues in the Middle East.) Syracuse, N.Y.: Syracuse University Press. 1991. Pp. xii, 177. $34.95.

Despite signs of what has to be called a recurring apologetic theme, Amira el Azhary Sonbol does a useful job of surveying successive stages of government programs for training medical doctors in Egypt both before and during the British occupation regime (1882–1922). About half of her discussion is devoted to the famous viceregal tenure of Muhammad 'Alī Pasha (1805–48). Study of developments during this reign, supplemented by much briefer analysis of the period of full-fledged penetration of Egypt by a wide variety of European influences (under Sa'īd and Ismā'il Pashas, between 1853 and 1879), provides the foundation for the thesis of the book. Briefly stated, Sonbol claims that, as the attributes of modern medical professionalism developed in Egypt, they "combined traditional modes with Western science to bring about a hybrid profession" (p. 133). In addition, readers receive the rather ambitious promise that, by reviewing these developments in the nineteenth century, they will gain a fuller understanding of "the importance of health and medicine in the religious, social and economic affairs of Islamic society" (p. 2). Unfortunately, with the exception of a general historical synopsis in the introductory section, this latter theme receives only token attention in later chapters.

Sonbol's discussion of the Muhammad 'Alī years confirms what a number of earlier scholars found: in the first period of autonomous military reform, decisions to develop Egypt's first modern medical cadres represented a practical extension of state political policy. Within this broad framework, the author's detailed and useful examination of the contributions of the French expatriate Dr. Antoine Barthélemy Clot (alias "Clot Bey"), founder in 1827 of the Abu Zabal (later Qasr al Aini) Medical School, tends to fill out a historical record that previously remained rather general. The same may be said for her coverage of another generally well-known Egyptian historical theme in this period, the place of medical studies in Egyptian educational missions to Europe during and after the Muhammad 'Alī years.

In estimating the original value of the compendium of material on the second half of the nineteenth century, there is much to learn from Sonbol's account that has not been published elsewhere. Despite the inherent interest of the material she offers, however, it is difficult to agree that the presumed thesis concerning "hybridization" is really substantiated.

Most of the information she provides focuses more on the administrative details of how medical training programs were run, who enrolled in them, and who ultimately qualified for graduation at what professional level, than on Sonbol's claimed analytical objective. The clearest example of this may be the fact that the discussion develops only one (in fact nonprofessional) subarea of medical practice in which such "hybridization" appears to be documentable. This involved the *hakimas*, traditional midwives who became the object of special training programs both before and after the British occupation in 1882. These programs trained them to perform various public health services beyond their main responsibilities of midwifery. Otherwise, Sonbol seems to rest too much of her apologetic case for Egypt's presumed potentiality for "continuity rather than change" on quantitative limitations in the late-nineteenth-century expansion of medical practitioners in Egypt imposed by budgetary cutbacks associated with the British-controlled administration (p. 108).

It is surprising to find that, although the author's bibliography contains, as one would expect, references to works on the history of the evolution of medical practices in countries other than Egypt, there is no mention of the clearly relevant book by Nancy E. Gallagher, *Egypt's Other Wars: Epidemics and the Politics of Public Health*, published in 1990 in the same Syracuse University Press series. What may appear to be a single, although important, oversight seems to point to a general characteristic in the use of secondary materials in this book. Many general supporting citations are from traditional, if not to say outdated, authors in Arabic, English, and French. Fortunately, primary sources, including the valuable memoirs of Clot Bey for the Muhammad 'Alī period, are well represented, providing most of the new information contained in the study.

BYRON D. CANNON
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BENNY MORRIS. *1948 and After: Israel and the Palestinians*. New York: Clarendon Press of Oxford University Press. 1990. Pp. xiv, 284. $69.00.

As Benny Morris points out in his preface, this volume is a collection of several of his essays that are intended to complement his earlier volume, *The Birth of the Palestine Refugee Problem, 1947–1949* (1987). Three of the nine essays included in this volume are

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new; the rest have appeared in *Tikkun*, *Middle Eastern Studies*, *The Middle East Journal*, and a volume titled *Refugees in the Age of Total War* (1988).

Collections of essays such as this have a common weakness: transitions from one chapter to another. The "flow" is often not smooth, and readers may feel as if they are, indeed, reading a collection of different essays by different authors rather than a single work by a single scholar. This is not the problem here. Chapter 1, on "The New Historiography," is introductory in nature and sets the stage for the rest of the volume by explaining Morris's perspective and his view of the purpose of "doing" history. Morris has added an addendum to this chapter (originally published in 1988), which responds to some of the criticism of his approach and perspective that accompanied the article's appearance and that of his *Birth of the Palestinian Refugee Problem*.

The rest of the chapters flow smoothly. They describe in varying detail the nature of the Palestinian "exodus" from the areas of Palestine, the ideological differences between the parties concerning this activity, the activities of Zionist officials dealing with the Palestinians, and the fate of several different Arab communities during the period under study. Morris also examines what happened to the Palestinian refugees once they left their homes and tries to explain why so many became what he calls "permanent refugees."

Morris's scholarship has generated strong responses from many historians, both Israeli and non-Israeli, because of the dramatic differences between his interpretation of events and what can only be called the "conventional" histories of the period. Morris pulls no punches, writing, for example, that "Israeli historians during the 1950s, 1960s, and 1970s were less than candid in their treatment of the Lydda-Ramle episode" (p. 3). He uses adjectives such as "deceitful" and "misleading" to describe what has become the "conventional wisdom" about this period, and his indictment of almost a generation of scholars disturbs many. It should be noted, however, that his scholarship appears to be solidly based on primary sources, some of which were apparently not available to the scholars who wrote the "conventional wisdom" histories, as well as other sources that we can only assume "conventional scholars" chose to ignore.

In his addendum to chapter 1, Morris discusses much of the give-and-take about his perspective, something that has variously been called a distinction between new historiography and old historiography, elsewhere referred to as "pre-history" and "history," and still elsewhere referred to as simply "pro-Palestinian history" (pp. 27–29).

For scholars interested in the Palestine of this period, this book should be considered must reading. Even if scholars decide that they prefer the "conventional" interpretations of history to that suggested by Morris, they will have trouble completely dismissing all of his arguments and, in any event, scholarship requires exposure to the widest possible range of argument.

GREGORY S. MAHLER
University of Mississippi

AFRICA

JOHN LAMPHEAR. *The Scattering Time: Turkana Responses to Colonial Rule.* (Oxford Studies in African Affairs.) New York: Clarendon Press of Oxford University Press. 1992. Pp. xxiii, 308. $72.00.

Some two decades ago, J. B. Webster of Uganda's Makerere University suggested that each of that country's numerous ethnic groups or tribes deserved to have its history recorded and preserved. A similar sentiment appears to be a primary motivation for John Lamphear. In this case, each group that resisted the imposition of colonial rule by armed force is deemed deserving of having its heroism and its tragedy recorded. I do not share this sense of the historian's obligation, but I am pleased to see that Lamphear has turned his skills to recording the resistance of the Turkana pastoralists of northwest Kenya.

The chief merit of this work is to provide the historian of Africa and imperialism with a finely crafted narrative of the conflicts and wars of conquest and resistance fought by British and Turkana armies in the first two decades of this century. Lamphear offers a new perspective on Turkana military and religio-political struggles, which until now have been treated solely from the viewpoint of the British conquerors. Extensive interviews with Turkana elders produced much valuable new material, not only on the conduct of the wars but also on the motives and values of the Turkana and their reflections on the complex responses to colonial penetration of their territory. From Lamphear's treatment of the documentary materials, one gets to know the personalities of the imperial officers more intimately than those of the Turkana. Still, intriguing portraits of key Turkana leaders emerge from the oral evidence; portraits, such as those of Ebei the war leader, Kokoi the Diviner of God, and Achuka the chiefly collaborator, are invaluable for their insights into Turkana culture and personality. Although often based on epithets and stereotypes common in oral historical traditions, these are much more than the African caricatures of colonial reports or imperial histories.

Another of Lamphear's purposes is to provide a military analysis of Turkana resistance, which was, after all, a matter of warfare, however exotic or small-scale. Here I find the author succeeds less well in his efforts. In the course of the narrative of Turkana resistance, Lamphear presents clear explanations of changing circumstances, both imposed from without and developing from within Turkana society, which led to various armed confrontations. Military actions, ranging from scattered, small-scale,

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and localized reactions to European (and African) raids, up to large-scale, coordinated cattle raids, planned by diviners and led by recognized war leaders, culminated in the emergence of a quasi-permanent military force based on Turkana age-sets. I can think of no other study of African resistance that offers such a panoramic view of African military developments. Occasionally, one may want more of an explanation of problems such as the paradoxical "coincidence" of the distractions of World War I with the final mobilization and onslaught against the strategically remote Turkana district. Similarly, the irony of the final defeat of the quasi-permanent Turkana army and the death of Ebei at the hands of the once friendly Dassanetch, rather than the British enemy, deserves more comment. Nevertheless, the military narrative is always engaging and often compelling.

Yet the "military analysis" offered in the conclusion does not provide a satisfying contribution to our understanding of African resistance. Any generalizations regarding the causes of anticolonial wars disappear with Lamphear's own presentation of exceptions and limitations to the analysis propounded. Focusing on the military aspects of Turkana resistance, the work properly belongs with the numerous studies of African resistance movements pioneered by T. O. Ranger and others in the post-independence era of the 1960s and popular through the 1970s, as reflected in the preponderance of titles from this period in Lamphear's footnotes. As such, this book seems a useful refinement and addition to the literature rather than a seminal or innovative contribution to an ongoing debate. In any case, we have been given a well-told story of a resistance struggle, well worth recording and preserving.

EDWARD I. STEINHART
Texas Tech University

ROBIN LAW. *The Slave Coast of West Africa 1550–1750: The Impact of the Atlantic Slave Trade on an African Society*. New York: Clarendon Press of Oxford University Press. 1991. Pp. viii, 376. $89.00.

The impact of the Atlantic slave trade on Africa has stimulated intense political, and therefore also academic, debate in Britain since the consolidation of systematic slaving there in the early eighteenth century. Robin Law's exhaustively researched study carefully reconstructs African events along and behind the aptly named "slave" part of the coast of western Africa, roughly the region from modern eastern Ghana to the Nigerian city of Lagos, during the formative years of the Atlantic trade in order to detect what seems to have been new by the 1720s and 1730s and may therefore, by contrast with earlier patterns, be attributed, at least circumstantially, to the growing volume of African-European commerce.

Law thus tests some of the classic propositions in African historiography by revisiting what is perhaps the most thoroughly discussed case study in the field, the famed militaristic and violent kingdom of Dahomey. British slave traders and abolitionists used the chaotic slaving of this state, and the statements of its accessible and articulate monarchs, as evidence in support of both sides of the essential conundrum: did slaving in Africa create surpluses of captives whom European merchants mercifully (if not humanely) saved from death, or did the Europeans, eager sellers of firearms and cynical buyers of human beings, stimulate the wars that wracked the region? This question has animated some of the great minds of twentieth-century scholarship—including the anthropologist Melville Herskovits and the political economist Karl Polanyi—to use Dahomey as a case study of their overarching theories of society and culture, and historians—Philip D. Curtin and others—have defined contemporary approaches to early Africa by examining other parts of the continent in search of pieces to fit in the same puzzle.

Law has applied the characteristic thoroughness for which he is respected from earlier work on adjoining regions to surpass all of his many predecessors in reconstructing the course of events there, mostly from about 1650 to 1750; the paucity of data for the preceding century defies even the persistence of a scholar who spent more than a decade researching and published more than fifteen technical studies as background for this monograph. Law's basic strategy of looking to see what changed sometimes stumbles on the lack of evidence for what happened before 1650, but the author is well aware of the logical difficulty and only occasionally drifts away from his solidly historical framework into the structural conceptualizations of the anthropologists who have previously influenced the field. The underlying empiricism of his approach does not lead to extended reviews of the theories behind his conclusions, but he offers informed, and precisely documented, comments on nearly all of them along the way.

What, then, was the impact of the Atlantic trade on the slave coast, according to Law? Access to the European traders' goods and, after about 1670, their firearms as well allowed African bandits and other violent challengers of the established order—including the warriors who created Dahomey—to spread destruction throughout the region. But the resulting depopulation from capture and flight, probably rather generalized, brought their raiding to an end for want of remaining victims and because the warriors who had captured early inhabitants became dependent on selling others for their own political survival. The violence of slaving moved inland, while slave traders gradually restored a form of order locally that, albeit autocratically, included private merchants and some aristocrats of the *ancien régime*. In terms of the underlying conundrum, proponents of both sides each perceived one piece of a story that

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makes sense only when grasped in its contradictory entirety. What was new? *Plus ça change, plus c'est la même chose.*

Law has given us as full and judicious a narrative of this general pattern along and behind the slave coast as anyone is likely ever to be able to provide, and in doing so he has confirmed, with a degree of documented likelihood rare in this field, a number of the elements of the broad consensus that seems to be emerging from thirty years of study of slaving by historians of Africa.

JOSEPH C. MILLER
University of Virginia

GWENDOLYN MIKELL. *Cocoa and Chaos in Ghana.* Paperback edition. Washington, D.C.: Howard University Press. 1992. Pp. xxvii, 284. $18.95.

Gwendolyn Mikell's book is a contribution by an anthropologist who during fieldwork studied firsthand the "chaos" caused by Ghana's policies toward its cocoa farmers. Although Mikell begins with a lengthy survey of the precapitalist economy and cocoa during the colonial period, her most important contribution is to view what the chaotic conditions caused by the punitive taxation of the industry meant at the local level. She undertakes an on-the-farm examination of the results of these policies, and the result adds to our knowledge of how African farmers and their families react to ill-advised output and input pricing structures imposed by governments.

For many years cocoa has been Ghana's leading export crop, and therefore the greatest source of foreign exchange. One of the best-known episodes of development economics is how President Kwame Nkrumah and his allies adopted a strategy of industrialization through import substitution, establishing state-owned and operated industries that frequently proved to be inefficient money losers. The government attempted to finance the strategy by taxing the cocoa industry, whose farmers had less political influence than city dwellers and were the one immediately available orthodox revenue source that could be tapped. The Cocoa Marketing Board's payments to farmers as a percentage of total export receipts at the official rate of exchange fell from 72 percent in 1960 to 41 percent in 1965, 37 percent in 1970, and 34 percent in 1983–84. With overvaluation of Ghana's currency, the cedi, taken into account, by 1983–84 cocoa farmers were receiving just 6 percent of the world price, valued at the prevailing parallel exchange rate. (For details, see M. M. Huq, *The Economy of Ghana: The First 25 Years* [1989], chap. 5 and p. 204; World Bank, *Accelerated Development in Sub-Saharan Africa*, Box A; World Bank, *Ghana: Policies and Program for Adjustment* [1984], 179–81; and D. K. Fieldhouse, *Black Africa 1945–80: Economic Decolonization and Arrested Development* [1986], 145.) The result was an alarming rise in subsistence production, a major cut in cocoa output, and considerable smuggling of what was produced across the borders to Togo and Côte d'Ivoire. Ghana had contributed 36.5 percent of world cocoa production in 1962–63, and that figure was still 30 percent in 1971–72. But a crop of 566,000 tons in 1964–65 had fallen to 225,000 tons in 1981–82, just 13 percent of world output, and downward still further to 159,000 tons, 10 percent of world output, in 1983–84.

Particularly for the Sunyani and Brong-Ahafo areas, Mikell describes and interprets the resulting tensions that arose between the genders, the fragmentation of lineage relationships, and the eruption of regional and ethnic antagonisms as cash income fell. At the village level, the social and economic effects she explores are fascinating, including the move into subsistence production by large numbers of people and some abandonment of settlements as villagers left to try their luck elsewhere. The attention to gender issues is noteworthy, as is the government's unwillingness to take a sufficiently large number of women and women's organizations under its political umbrella. The effects on poor trading women during the period of cocoa chaos could be poignant, as when the declining availability of goods to trade plus the attempts to control prices forced these women into activities that required less financial capital than before. Mikell notes the struggles that developed between husbands and wives over household budgets as incomes declined, as well as the detrimental effects on children. The impact of government policy will be of interest to economists, economic historians, sociologists, and political scientists, in addition to anthropologists.

The introduction to the book contains a certain amount of anthropological jargon, and the opening chapters on the economy of the area in earlier centuries could well have been shortened. But readers should persevere, because the central message of this book ought to be understood by all who advise on or carry out government policy in agriculture.

JAN HOGENDORN
Colby College

CORA ANN PRESLEY. *Kikuyu Women, the Mau Mau Rebellion, and Social Change in Kenya.* Boulder, Colo.: Westview. 1992. Pp. xiv, 213. $32.00.

Cora Ann Presley's work is a welcome addition to the small but significant collection of book-length historical studies of African women published in the last twenty years. Presley focuses on changes in Kikuyu women's lives in Kiambu district during the British colonial period, 1880 to 1962, with particular attention to the growing significance of their political activities in the 1920s and 1930s. Historians of Kenya agree that these were pivotal decades of political organization in response to intensifying economic exploitation as increasing amounts of prime land

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were alienated to white settlers. But in failing to explore the ways in which Kikuyu women engaged the British colonial state in these years, most scholars have missed a crucial dimension of Kikuyu politics and response to economic hardships, and it is this dimension that Presley examines.

Kikuyu women were, for example, ardent activists on behalf of Kenya's first nationalist politician, Harry Thuku. Moreover, as angry laborers in European coffee plantations, they struck to register their grievances: poor wages and working conditions, including physical harassment. Presley ably brings into focus these and other important examples of women's involvement in shaping Kenya's political and economic history through careful archival work, and through oral interviews with three groups of women in Kiambu district: church members, rural women's self-help and cultural groups, and, most significantly, political activists, including a number of Mau Mau ex-combatants.

In addition to exploring women's politicization around issues of land, labor, and cultural nationalism (particularly the notorious battle over clitoridectomy in the late 1920s), Presley examines the differences between women's and men's political actions, and between loyalist and nationalist women of the Mau Mau period (1952–57). In two insightful chapters on Mau Mau, Presley argues that Kikuyu women's participation was central to this movement, effectively demonstrating that the British administration's "victory" over the nationalists coincided with the imprisonment of large numbers of women and the effective severing of supply and communication ties between women and men militants through the virtual confinement of thousands of Kiambu Kikuyu in "protected" (that is, enclosed by barbed wire) villages.

Because Kikuyu women had not, except for special women's councils dealing with women's affairs, participated in the formal political structures and processes of their communities, Presley insists that their entry into anticolonial political organizations from the 1930s "changed the Kikuyu social contract" (p. 3). Moreover, both radical nationalist activity and, ironically, the anti–Mau Mau government community development efforts designed to "domesticate" women's political activities (the Club Movement, home economics and baby care, and so forth) created the conditions enabling elite Kenyan women to hold positions in the postcolonial government since independence in 1963. In this slim but important volume, Presley has provided Africanists and other historians, as well as all interested readers, especially Kenyan women, with ample evidence of an important historical role for Kikuyu women during the colonial period.

SUSAN GEIGER
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COLIN MURRAY. *Black Mountain: Land, Class and Power in the Eastern Orange Free State, 1880s to 1980s*. Washington, D.C.: Smithsonian Institution. 1992. Pp. xiv, 340. $49.95.

At sunup on a July morning in 1884, Chief Tshipinare of the Barolong, a people inhabiting the southern highveld of South Africa, was murdered. His killing marked the culmination of a bitter civil war waged over succession to the office of the venerable Barolong chief, Moroka, who had settled his people during the 1830s on a spacious and fertile tract of territory west of the Caledon River in what was to become the Orange Free State (OFS). The territory was named Thaba Nchu, "Black Mountain"; this book is the chronicle of that region and of its people, both African and European, from that time down to the present.

According to Colin Murray, Chief Tshipinare's murder, besides being poignantly humiliating (he was shot and stabbed as he fled from hiding in the royal latrine), was of enduring consequence. It provided the excuse for the neighboring OFS government to step in and annex the Barolong territory of Thaba Nchu (rather, it claimed, than see it fall to Tshipinare's rivals and murderers), and then to parcel it out. Two-thirds of the land went to Africans (who before 1913 could own land legally), the remainder to individual whites (many of them Tshipinare's creditors), to mission societies, and to the OFS government itself.

The essence of this book, and its genius, lies in its encapsulating much of what is important in the history of South Africa over the past hundred years in its description of what became of Tshipinare's former holdings and their inhabitants. Most of the land that fell to the Barolong under OFS dispensation was gone within a generation to whites, sold off either to pay debts and mortgages or to enable Tshipinare's heirs to seek better fortunes (they thought) in far-off lands. Although it was still lawful for Africans to own land, Murray describes how already in the late nineteenth century the market was rigged against African tenure, so burdening their lands with servitudes and levies as to force many of them to sell into a market restricted to European buyers only.

So it went throughout the twentieth century. Droughts and locusts ruined crops. Postwar imperial occupation schemes after 1902 brought British veterans and other settlers to the lands and then subsidized them (subsidized, in fact, all white farmers) to the disadvantage of the Barolong for the remainder of the century. Then came the passage of the Natives Land Act of 1913, the effects of which were staggering. Not only did it dispossess most Africans of their lands but it also vastly weakened the bargaining power of African sharecroppers, so that only a handful of educated, elite Barolong managed to hold on to what remained of Tshipinare's former holdings. They and their survivors found their titles in the

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progressively oppressive political climate of twentieth-century South Africa to be of mixed benefit. Some mortgaged the land to save it from predatory whites, farming it in most cases with marginal success or none at all; others squandered it in pursuit of excess before they died. A few had the prescience to make use of its liquidity as the "material springboard for a decent education and thence for socio-economic mobility" (p. 283), some of them with spectacular success as professionals, entrepreneurs, and politicians. One of them, James Sebe Moroka, a wealthy medical doctor and farmer, became president of the African National Congress from 1949 to 1952.

But those were the exceptions. The vast majority of twentieth-century Barolong suffered the fate of most Africans, uprooted in the name of "Separate Development" and subjected to a nightmare of dispossession, endemic rootlessness, hunger, and despair in the communal Bantustans that became the kernel of apartheid policy. Consequently, many of Tshipinare's lands eventually were turned over to such territories, even incorporating by the 1980s such notorious rural slums as Botshabelo, "Place of Refuge" for half a million people who had been cruelly extruded from the towns and farms of what was by then "white" South Africa.

By examining exhaustively what has happened to Thaba Nchu and its people over the last century, Murray has made an exceptional contribution on several levels. South Africa is on the threshold of a long process of redistribution of power, wealth, and land, and it is extremely useful to know in detail how one small corner of it became so maldistributed. For what happened to Black Mountain and its inhabitants happened to much of South Africa. Murray's tracing of its history is exhaustively researched and evocatively described in all of its complexities. And the final question that he posits concerning Thaba Nchu's tangled circumstances constitutes the universal, Gordian challenge for the next generations of all South Africans: "Whose land, indeed?"

ALAN R. BOOTH
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RICHARD WERBNER. *Tears of the Dead: The Social Biography of an African Family.* (International African Library.) Washington, D.C.: Smithsonian Institution. 1991. Pp. xi, 211. $19.95.

This history of a Kalanga family in southern Zimbabwe would not have been possible, as Richard Werbner notes, if his own history were not part of it. Werbner has engaged in anthropological research in southern Africa since 1960, visiting the Lupondo family before Zimbabwean independence and after both the war of independence and the subsequent civil war in the 1980s. The long view in this work has also the breadth of Werbner's books and articles. His story is one of appalling brutalities and privations tempered by the extraordinary moderation, reasonableness, and stoicism of the family's recollections and commentaries, their accounts translated into English in a way that preserves the flavor of the original idiom without ever burdening the reader. This is a book about memory, and a book about how the living reconcile themselves with the dead and with death itself.

This is a history of Zimbabwe, formerly Rhodesia, seen from the point of view of people who, in the face of what Werbner calls the quasi-nationalisms of the day, were always marginalized and therefore victims. But it is also the wider history, enriched by documents, of the same events from a perspective that could not be known to them. The story begins with the Rhodesian policy of displacing Africans from the best land, followed by a paternalistic and pseudo-rational but fundamentally muddled policy of development. Werbner draws on the official record to describe the contradictory pressures on the government's appointed chief in the area; we then hear the voices of the Lupondo family, telling of their experience of the Europeans. Then the focus shifts, in the third chapter, to the individual recollections of family members: the quarrels and good times, the births and deaths. Men and women, and the young who become old in the course of the story, have different views of events and different moral judgments concerning them. Some voices are missing, marking those who died prematurely.

Since some members of the family moved to other parts of the country and from rural to urban areas, their stories tell us about much more than the village that is the main focus. In the last chapter, "Away from Home," Werbner draws mostly on the stories of women of the third generation, now living in Bulawayo. The beatings and killings of successive wars contrast poignantly with the strength and buoyancy of the women telling about them; their repeated comment that "Smith [of the white Rhodesian regime] was better" (p. 185) shocks Werbner into reassessing some of his own convictions. Werbner writes with full consciousness of his craft as a teller of stories, and of stories within stories, but also as an anthropologist, telling us that "the book has to move between different kinds of narrative: one that documents moral sentiment and passion; another that examines or analyses such documentation; still another that argues, explains or criticises as it unfolds the context of other narratives" (p. 5).

In Zimbabwe, the experience of senseless loss and anxious bereavement has created a national feeling of need to effect cleansing from the violence of the wars. "Tears of the dead" is a Kalanga expression that refers both to mourning and to the inheritance in goods and cattle that the living receive from the dead. As indigenous culture is increasingly converted into commodities for sale as curios in town and as gifts in the country, the living reestablish relations with their

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parents and grandparents, hearing their voices in the trances of spirit mediums, many of them women. The cult of the dead is a living memory, a way of reflecting on the past and the present, a healing exercise.

This deeply moving book is excellent as both history and as anthropology. It should be read not only by those who have a professional interest in Zimbabwe or Africa but also by those who seek to know the human history of our times. The story of the Lupondo family could have come from Liberia, Bosnia, or Afghanistan.

WYATT MACGAFFEY
Haverford College

ASIA

JOHN KING FAIRBANK. *China: A New History*. Cambridge: Belknap Press of Harvard University Press. 1992. Pp. xvii, 519. $27.95.

It will surprise no one who knew John King Fairbank, even if only by reputation, to discover that he delivered the manuscript of this book to the press on the day that he suffered a fatal heart attack. Seriously ill since 1989, he not only continued work on this book but also began two others. Such was Fairbank's determination and dedication to the field of Chinese studies.

Fairbank's aptly titled study displays his many virtues as a scholar: his broad vision; his persistent inclination to view China in cross-cultural as well as long-range historical perspective; his relentless absorption with the most interesting and significant issues of Chinese history; his command of a vast and rapidly growing corpus of relevant literature in a number of different disciplines; his ability to synthesize the more narrow scholarship of others into large and meaningful generalizations, without sacrificing a sense of complexity and nuance; and his crisp, lively prose style.

Not surprisingly, much of Fairbank's book follows the intellectual contours of his earlier work. It reflects, for example, his preoccupation with the complex relationship between the agrarian life style of China proper and the pastoral nomadism of the Inner Asian frontier. It also devotes a great deal of attention to Chinese social and political organization over the past three thousand years. In all, Fairbank remains more interested in administrative thought than in religious beliefs, more taken with social and economic issues than with art, music, and literature. His is far less a cultural history than a political history. Moreover, Fairbank has always had a special interest in the modern era; hence one often gets the feeling that China's early past serves simply as a prelude to the present. Nearly half of the book's 432 narrative pages focus on the twentieth century.

Yet this study is indeed a "New History," and not simply because Fairbank has drawn so extensively from recent scholarship. One of the most refreshing features of the book is the author's willingness to reevaluate some of his own positions and to address some of the less attractive features of Chinese social and political life, including the long-standing institutionalized use of violence by the Chinese state and the unique practice of footbinding, which every year for a millennium or so crippled millions of female children. In these and other instances Fairbank distances himself from what he calls "sentimental sinophilia" (p. 176)—the tendency for China specialists to love China and therefore avoid making it seem sordid.

The book is a joy to read. It takes us from the dusty archaeological remains of paleolithic China to the tumultuous present of the People's Republic (and Taiwan) with breathtaking speed and unremitting clarity. Throughout this splendid journey Fairbank treats China as a part of world history, offering stimulating comparisons designed to show the ways China's experience had been both like and unlike that of other parts of the world (primarily Europe and America). At the same time, he devotes considerable attention to regional differences within China, drawing inspiration from G. William Skinner's pioneering work on marketing and urbanization within the context of various "macroregional" river drainage systems. This approach, he remarks, has been "a great stimulus to local history," and has "already produced a number of outstanding studies" (p. 436).

In the end, however, Fairbank seeks an integrative analysis, one that considers "China" to be a manageable and meaningful generalization, despite its enormous size and cultural diversity. One obvious reason for this view is the persistent belief, shared by both Communist cadres in the present and Confucian scholar-officials of the past, that China is a unitary state. Whether for better or worse (Fairbank implies for worse), China boasts the world's longest tradition of "successful autocracy." "Never," he writes, "have so few ruled for so long over so many" (p. xvi). The institutional and ideological devices of imperial China—inherited in rapid succession on the mainland by the Nationalists and then by the Communists—created a self-sufficient, self-sustaining, and, on balance, enormously impressive civilization, but one poorly suited, in Fairbank's view, to "lead the way in modernization" (p. xvi).

Fairbank clearly considers "modernization" to be a good thing, but he does not define the term. He perhaps has in mind not only the systematic use of advanced technology but also the Western concept of a "civil society"—some type of political pluralism in which human rights and civil liberties are maintained under the supremacy of law. Yet Fairbank does not hold to the naive view that China should simply take the West as its cultural model. He notes that "We outsiders can offer China advice about the overriding need for human rights, but until we can set an example by properly curbing our own media violence and the drug and gun industries, we can hardly urge China to be more like us" (p. 432).

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This is vintage Fairbank. He is part preacher, but his sermons are designed to enlighten rather than simply to persuade. What distinguishes this book is its balanced perspective. One can always pick nits; I would like to have seen more attention given to Chinese mythology (which, until the twentieth century, the Chinese considered to be "history") and to philosophy (especially the varieties of thought in both the Warring States period and the late imperial era). But these are minor criticisms. Overall, Fairbank has produced a magnificent book, one that will delight general readers and specialists alike.

RICHARD J. SMITH
Rice University

STEVEN F. SAGE. *Ancient Sichuan and the Unification of China.* (SUNY Series in Chinese Local Studies.) Albany: State University of New York Press. 1992. Pp. xvi, 320. Cloth $68.50, paper $22.95.

The geographic isolation of Sichuan, today China's most populous province, has always shaped the distinctive contours of its history. While lying within the cultural horizon of North China's neolithic and bronze civilizations, Sichuan also remained a world apart, at least until its incorporation into the expanding Qin empire in the fourth century B.C. Yet because Sichuan rarely appeared in the annals of Shang and Zhou China, its early history has been cloaked in mystery and myth. In this book, Steven F. Sage exploits the welter of recent findings by Chinese archaeologists to reconstruct the origins of civilization in Sichuan and the process whereby Sichuan was eventually integrated into a unified Sinitic polity under the Qin empire.

Sage introduces to us the exciting discoveries at Sanxingdui, a major settlement site near modern Chengdu that spanned the second millennium B.C. In addition to being an important urban center dating back to the early to mid-Shang period, Sanxingdui—at least in its later stages, dating to the late-Shang–early Zhou period—has revealed an apparently indigenous bronze culture. In the absence of written records we cannot confirm the existence of Shang-style political institutions at Sanxingdui, yet these findings testify to the emergence of at least a protostate (perhaps similar to predynastic Zhou?) in Sichuan prior to the founding of Zhou.

In the aftermath of the Zhou conquest of Shang, however, Sichuan became increasingly isolated from the cultural influences of North China. No discernible links between the Sanxingdui polity and the Warring States–era kingdom of Shu have yet been found. Indeed, the earliest documented relations between Sichuan and the Zhou world involved the more primitive kingdom of Ba, in eastern Sichuan, which became embroiled in the complex politics of the contending Spring and Autumn-era states in the early seventh century B.C. Withdrawing westward in the face of aggressive expansion by Chu, Ba drew closer to Shu. By the fifth century B.C. intercourse between the two states produced a hybrid Ba-Shu culture, with its own distinctive material civilization, artistic styles, and as yet indecipherable written scripts.

In the second half of the book Sage retells, from the perspective of Sichuan, the story of the unification of China under the Qin and Han empires. Aside from the off-center perspective, there are few revelations in this part of his study. Sage relies entirely on traditional annals such as *Shiji* and *Huayang guozhi* to detail the conquest of Shu and Ba by Qin in the fourth century B.C. and the "sinification" of Sichuan under Qin and Han rule. Sage fails to make a persuasive case for his claim that the "consequences [of the Qin conquest of Shu in 316 B.C.] rank this date among the most memorable in Asian history" (p. 83). He contends that the conquest of Sichuan marked the decisive turning point that set Qin on the road to world domination. Although it is undeniable that the conquest of Sichuan augmented the material and human resources of Qin, Sage does not demonstrate that it made any unique contribution to the political economy of Qin or the process of empire building. Weighty declamations that "Shu served as a political laboratory" for the development of Qin territorial administration (p. 130) or that under Qin rule Sichuan witnessed the "rapid spread of specie" and the emergence of a "rising, dynamic class of merchants" (p. 135) rest on only the most slender pillars of evidence.

Sage's analysis all too often is warped by his own boosterism. He at once argues for the distinctive cultural origins of Sichuan and the crucial role (one is tempted to say destiny) of Sichuan in the unification of the Chinese world. Although on occasion Sage exhibits discrimination in his use of legendary accounts of early Sichuan history, his effort to distill historical fact from the mythology of the third century A.D. work *Huayang guozhi* would benefit from an analysis of the text itself, which to my mind is governed by a desire to portray Sichuan as an integral part of the Chinese world. Sage's study largely reiterates the familiar teleology—still the keystone of the concept of nationhood espoused by China's rulers today—of Chinese unity: however diverse in their origins, China's regions and peoples inevitably evolved to form a "natural" unity. Regrettably, Sage offers little, either in empirical findings or theoretical analysis, to embellish traditional accounts of state-making in early China.

RICHARD VON GLAHN
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ALAIN PEYREFITTE. *The Immobile Empire.* Translated by JON ROTHSCHILD. New York: Alfred A. Knopf. 1992. Pp. xxvii, 630. $30.00.

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Alain Peyrefitte's long and ponderous book attempts to interpret China and its modern fate through the lens of George Macartney's mission to China (1792–94). He contends that the confrontation between Great Britain and China over diplomatic and audience ritual resulted primarily from Chinese cultural conceit that has stifled China's latent dynamic impulses and kept it bureaucratic, backward, and feeble ever since. Although Peyrefitte acknowledges that British arrogance contributed to the failure of the Macartney mission, he returns over and over again to shortcomings in China's values and institutions that formed an impenetrable barrier to its successful transformation. But to what? Presumably to becoming a mirror image of the triumphant, dynamic, free-trading West.

The book's 550 pages are divided into six parts. The first covers the actual outbound trip from England to Macao (September 1792 to June 1793) and tries unsuccessfully to weave in an account of Europe's prior knowledge about China, the goals of the mission, information about Macartney's parvenu origins, and the mission's role in the British free-trade movement. The second part covers in onerous detail the journey from Macao to Peking (June to September 1793), with emphasis on issues and events that foreshadowed the failure of the mission, such as trade restrictions in Canton and arguments over political and diplomatic ritual. The third section follows the events leading up to and including the audience with the Qianlong Emperor in Jehol (September–October 1793); the fourth and fifth parts record the mission's return journey to Canton via the Grand Canal and overland route (October 1793–September 1794). The sixth and final part discusses the Sino-Western confrontation from the failed Macartney mission to the Republican revolution in 1911. Peyrefitte concludes that China not only missed the opportunity offered by the Macartney mission but also every subsequent chance to respond to and join with the West in the last two centuries of development and progress.

The work has little narrative coherence, thematic development, or interpretive originality. Although Peyrefitte claims to explore new source materials from the Qing archives in Beijing and the diary of young Thomas Staunton, Macartney's eleven-year-old page, none of it adds significantly to the historical analyses presented years ago by J. L. Cranmer-Byng ("Lord Macartney's Embassy to Peking in 1793," *Journal of Oriental Studies* 4 [1957–58], 117–83) and E. H. Pritchard (*The Crucial Years of Early Anglo-Chinese Relations, 1750–1800* [1936]; "The Kowtow in the Macartney Embassy to China in 1793," *Far Eastern Quarterly* 2 [1943], 163–203). Although Peyrefitte ignores important differences between the East India Company and the emerging free-trade movement, his most serious omissions appear in the treatment of Chinese history and civilization. He fails to consider the nature and internal logic of Chinese values and institutions, and he appears to have ignored nearly a half century of scholarship that sheds light on important and dynamic aspects of late-imperial Chinese history, such as the boundaries of power and the prerogatives of the late-imperial state, the integration of the Inner Asian border regions into the empire, the commercial revolution, and the evolution of municipal government from private subdistrict organizations. None of this seems to penetrate Peyrefitte's Eurocentric critique of China.

The narrative is weighed down by stifling, trivial detail on the day-to-day events of the mission, punctuated by the repetition of tired clichés about China and the Chinese and by the interjection of Peyrefitte's personal observations and experiences as a traveler and diplomat in China. These personal insights are somehow intended to document and verify the correctness of his interpretations, and they range from the incredible assertion that Chinese are "insensitive to the death of others" (p. 99) to a Freudian interpretation of the culture that has the Chinese "mired . . . in a psychic infantilism that meshes perfectly with their xenophobia and their rejection of innovation" (p. 147). This hodge-podge narrative is further fragmented by Peyrefitte's attempt to place the mission in a sweeping Braudelian framework by referring to contemporaneous but unrelated events in European history. The result is a pretentious and undigested work of limited antiquarian interest.

JANE KATE LEONARD
University of Akron

EMILY HONIG. *Creating Chinese Ethnicity: Subei People in Shanghai, 1850–1980*. New Haven: Yale University Press. 1992. Pp. xv, 174. $22.50.

This is a boldly conceptualized book about the prejudice of Chinese against members of their own race: "Subei people" who were immigrants into Shanghai from the poor rural counties of northern Jiangsu. Alerted first to this ethnic-like bias while interviewing Shanghai residents in the 1980s, Emily Honig supplements subjective accounts of native-place–based contempt with sociological data that show a pervasive pattern of discrimination since the mid-nineteenth century. The "Subei phenomenon" was borne out by the 1920s in the overwhelming concentration of northern Jiangsu natives in Shanghai's lowest-paying jobs and in the shack slums along the city's periphery. Honig contends that bias by Jiangnan ("South of the Yangzi") natives against Subei immigrants went well beyond contempt for an economic underclass. Even though racial differences were not present as a factor, this intolerance amounted to a case of ethnic prejudice that was firmly linked to a popular Shanghai belief in the inferior culture and dull intelligence of Subei folk, who were singled out by their coarse language, gawdy clothing, and poor hygiene.

The author shows that the category "Subei people" appeared in Shanghai during a period of rapid

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industrialization and Westernization, when massive immigration from economically stagnant rural areas north of the Yangzi into the treaty port took place. But the alleged homogeneity of "Subei" and the "Subei people" lacked objective foundation in the actual geographical and cultural conditions of the region. It was instead a Shanghai construction invented by the city's Westernized economic elites. "To the extent that foreigners portrayed all Chinese as uncivilized and backward . . . Jiangnan natives may have more desperately than otherwise needed to create an other" (p. 132) in the coolies and rickshaw pullers of northern Jiangsu origin. Honig, however, stops short of presenting the construction of Subei ethnicity as another instance of China's response to the West. Nor does she see it as simply a modern version of the age-old acrimony between migrant and indigenous communities extensively documented in Chinese history. Instead, she sketches out both possibilities for her readers, leaving the answer to future researchers in this sensitive field.

This book brings to light with admirable competence a previously overlooked dimension of modern Shanghai's social cleavages. But it achieves greater success in its description of the Subei phenomenon than in its explanation of how it came about. Was it a matter of their own preference or was it the result of an imposed segregation that Subei migrants to Shanghai clustered into neighborhoods of their own? Honig seems to think that it was both. Although the agents of ethnic construction are presumably Shanghai's "Jiangnan elites," nowhere in the book are these elites actually shown in the act and process of producing Subei ethnicity. Nor is the assumed homogeneity of "Jiangnan" itself, no less an evolving cultural construct than "Subei," ever subjected to a parallel analysis. Both of these congeries of native-place ties, of course, were forces fluctuating across the field of Chinese national identity—itself a product of vigorous construction by Yangzi delta middle-class elites. Honig showed us in her first book, *Sisters and Strangers* (1986), how the murder of a Subei cotton-mill worker by a Japanese guard served to mobilize the May 30th Movement of 1925, which was one of the most dramatic moments of Chinese nationalist protest in Shanghai. Here that same worker's death is noted instead for giving occasion to a memorial service sponsored by fellow townspeople from Yancheng and not by mourners from other parts of northern Jiangsu (p. 80)—thus an instance of localism that Honig uses to illustrate the fabricated nature of the larger Subei identity. Honig's precisely articulated emphasis on Jiangnan elites' construction of Subei ethnicity is rightfully designed to expose a new dimension of social injustice, but some measure of historical complexity is nonetheless forfeited along the way.

WEN-HSIN YEH
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GREGOR BENTON. *Mountain Fires: The Red Army's Three-Year War in South China, 1934–1938*. (Philip E. Lilienthal, series 7.) Berkeley and Los Angeles: University of California Press. 1992. Pp. xlv, 639. $70.00.

Ever since Gregor Benton presented his pioneering paper "Communist Guerrilla Bases in Southeast China after the Start of the Long March" at the Conference on Communist Rural Bases in 1978, Chinese Studies students and scholars have been waiting for the book. But after a decade, one tends to lose hope and think that perhaps there was only enough evidence for that essay, which was later published in Kathleen Hartford and Steven Goldstein's *Single Sparks: China's Rural Revolutions* (1989).

Fortunately, there proved to be adequate material to complete the study, only it took an inordinate amount of time to gather it. Benton should be commended for having the persistence to unearth materials in archives in Taiwan and the patience to wait for the winds of change to make available pertinent sources in the People's Republic of China. Among the primary materials he gathered, memoirs by veterans of the "Three-Year War" published by provincial and local presses during the 1980s proved indispensable in reconstructing that significant period of modern Chinese history. Realizing that the use of memoirs is fraught with difficulties, Benton devotes a section of his preface to preempt criticisms of their use, specifically their inherent incompleteness and unreliability. At the same time, he points to their usefulness when handled in a judicious manner. It is the sort of discussion that would benefit any fledgling scholar pondering the scarcity and utility of sources for the study of Chinese history. His insightful remark that "in China, even people remembering independently of one another [may] sometimes commit identical untruths because they are subject to the same internal constraints" (p. xxi) will undoubtedly find its way into lectures on historical methods.

Benton's encyclopedic study is well worth the wait. It is a monumental work that reconstructs in minute detail the horror and heroism of the "Three-Year War," a conflict that was waged in fourteen different places in South China. Naturally, he begins his narrative with the defeat of the Chinese communists and ends with the establishment of the New Fourth Army, a singular strategic triumph barely mentioned in most history books. Besides rescuing the "Three-Year War" from obscurity, he places it in proper historical perspective, that is, alongside the better known Long March. In so doing, Benton joins in the political dialogue in the People's Republic of China and the scholarly discourse in the field of Chinese Studies on Mao Zedong's legacy. His study shows beyond a doubt that there was more to the Chinese Revolution than Mao leading the peasant masses through Sturm und Drang.

At the center of the book are short histories of each communist base and its place in the protracted strug-

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gle that was the "Three-Year War." Of these, "Min-xi'nan: Mountain Marxists" is the most complete chapter as well as the best documented, with an astounding 248 notes. From these histories, Benton concludes that the key to surviving the isolation and repression that followed the fall of the Jiangxi Soviet was to assume a pragmatic stance and adapt to local circumstances rather than to adhere dogmatically to irrelevant ideological prescriptions or inappropriate revolutionary strategies and tactics. That is why Benton takes such pains to describe the customs and habits of the people in each region. Indeed, he does this with such verve that ethnographers could learn a lesson or two from him. Anyone wishing to understand how and why insurgencies manage to survive under the most arduous conditions would do well to read these individual studies.

Fascinating though they are, these histories may appear redundant. Whereas each story is unique in its specifics, the overall plot is one of initial defeat and ultimate survival, which in this context is the equivalent of victory. Furthermore, the rich details may prove daunting to the uninitiated. Those unfamiliar with the period may find the plethora of names and events overwhelming. A chronology of the communist revolution from 1921 to 1949 and an appendix listing the main military and political leaders helps somewhat to mitigate this. Perhaps a more effective way of discussing the "Three-Year War" would have been to analyze its salient aspects across time and space rather than providing a short history of each communist base.

These shortcomings are small matters and hardly diminish Benton's achievement of producing the definitive history of the Red Army's "Three-Year War" in South China.

WILLIAM WEI
University of Colorado, Boulder

JOHN WHITNEY HALL, editor. *The Cambridge History of Japan.* Volume 4, *Early Modern Japan.* Assisted by JAMES L. MCCLAIN. New York: Cambridge University Press. 1991. Pp. xxviii, 831.

From the bulk of its individual volumes to the series' avowed purpose ("to put before the English-reading audience as complete a record of Japanese history as possible," [p. vii]), the *Cambridge History of Japan* announces grand aims. The present volume covers early modern Japan, here defined as the period from 1550 to 1800. Its fourteen essays by prominent Japanese and American specialists are fairly evenly divided between those that cover the consolidation and first century of Tokugawa rule and those that treat the eighteenth century. All offer detailed, scholarly discussions of subjects that have become *de rigueur* for the period: the wars of unification, the rise of the *bakuhan* system, the "Christian century," Neo-Confucian and nativist thought, commercialization, and the development of an urban, merchant culture.

As these topics suggest, the subject matter is skewed toward the traditional. A similar tendency governs the kinds of history chosen to treat early modern Japan. Institutional and political history predominate, occupying nine of the fourteen essays; intellectual history is afforded two chapters (thought in the seventeenth and eighteenth centuries, respectively). A discussion of the period's economic trends is related in one lengthy chapter, and culture brings up the rear, with essays on material culture and popular culture rounding out the collection. As we have come to expect from Cambridge histories, the essays strive not to break new ground but to consolidate the field. Thus, the institutions receiving the most coverage are the *bakufu* and the *han*; culture consists of kabuki and the floating world; all of Tokugawa Japan, save for the pleasure quarters, is devoid of women. The reader who opens this volume hoping for fresh viewpoints or new information will, with a few welcome exceptions, be disappointed. The volume enshrines an already well-established understanding of early modern Japan.

Even allowing for the work's conservative mandate, the essays in the first half of the volume seem stale, perhaps because we already have two conference volumes (featuring many of the same authors) devoted to the period of unification. Of course, it would be hard for a general history of Sengoku and early Tokugawa Japan to avoid revisiting such common ground as the rise of the Tokugawa family or the development of the *bakuhan* political structure. But do we really need another long (seventy-three pages) rumination on the Christian century that was not? Does it make sense to have separate articles on the *bakufu* and *han* when it is their intertwining that characterizes Tokugawa governance (a fact that also makes for a good deal of repetition between the two pieces)? Often, too, the authors seem to have interpreted the demands of the Cambridge genre as requiring either voluminous, blow-by-blow narrative or encyclopedia-like coverage. Thus, the chapter on relations between Japan, China, and Korea turns a potentially fascinating and little appreciated aspect of Tokugawa history into a recital of embassies and trade statistics. And, exemplifying the latter strategy, the essay on seventeenth-century thought offers thumbnail sketches of all the major Tokugawa philosophers, but no synthesis.

The eighteenth century fares better. The authors in this section seem less concerned with "covering the field" and more interested in historical argument. Tetsuo Najita's contribution on eighteenth-century thought, for example, is stimulating precisely because it does not offer simply a series of sketches of Tokugawa philosophers. Instead, the imperative to cover the major thinkers has been skillfully woven into an argument about the articulation of history and nature in Tokugawa thought. James McClain's

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collaborative effort with Nakai Nobuhiko likewise illustrates what this kind of writing can be at its best. Comprehensive and informative, this essay, too, has a point to make about the economic and urban "revolutions" of the period, not just a topic to cover. It ventures into local history (despite this being, according to the introduction, a territory off limits to the Cambridge History) to provide a perspective on early modern urbanization and commercial growth that has not made it into the English-language mainstream.

Perhaps it is the genre, perhaps it is the editorial policy, but the authors' immense expertise about the period does not come across in a stimulating or thought-provoking way. The volume is best, in fact, where the authors have strayed from their appointed task of summarizing Tokugawa history and where, especially, they have not taken as self-evident the importance of their topic. Such instances, however, are relatively rare; for the most part, this is history written in the monumental mode, a history that seeks to claim and secure a realm, not one that hopes to open it up. This aspiration betrays itself by the frequency with which the question of who owns Japanese history crops up. The assertion, "Japanese history belongs to the world," of the general introduction paves the way for a recurring contest over who—Western or Japanese historians, Western or Japanese concepts—may lay claim to what. Thus, John Hall's brief history of the "Edo field" is in fact an account of the inroads made by Westerners on a once secure Japanese bastion: "by the 1960s . . . Western scholars trained to use Japanese primary materials began to make their presence felt" (p. 35). And this assertion is echoed by Wakita Osamu's remark that "American scholarship, of course, has been influenced by . . . Japanese researchers. Yet it is important to note that Western historians have . . . independently developed their own frameworks of analysis" (p. 98).

Ultimately it is this (perhaps unconscious) desire to claim Japan for Western (more explicitly American) scholarship that gives the volume its distinctive tone. And although it may inspire a sense of achievement to say that "we can do it as well as they can," one wonders why we still need worry about which essence, the West or Japan, is empowered to produce knowledge about "Japan." Is it not time that this question, though constitutive of so much of Japanology, was laid to rest?

THOMAS KEIRSTEAD
McGill University

CHETAN SINGH. *Region and Empire: Panjab in the Seventeenth Century*. New York: Oxford University Press. 1991. Pp. x, 345. $29.95.

The contribution of Chetan Singh's book to the history of Mughal India stems from its departure from the traditional historiography on medieval Panjab. The conventional approach takes Sikhism and the Sikh-Mughal struggles as the defining themes of the period. The virtue of Singh's treatment is to ignore, for the first six chapters at least, religious topics in favor of administrative, agricultural, urban, and commercial ones. This feature of the book places Singh securely in the small but growing camp of Mughal historians who take the regional and local rather than the subcontinental view of medieval India.

His discussion of various aspects of economic activity in the Panjab during the seventeenth century is particularly useful and informative. Singh provides a degree of detail hitherto unavailable on agricultural developments in irrigation, crops, and commercialization; the maturation of towns and cities as centers of production and exchange; and increases in the quantity, variety, and routes of commercial activity. In contrast to the freshness of the economic chapters, however, the chapters on administration and zamindars seem stale, too dependent on the standard sources. Here Singh fails to enliven the imperial perspective with a viewpoint distinctive of the Panjab.

The strength of Singh's book is also the source of its principal weakness. The author appears to have gone too far in his effort to avoid the established religious framework of Punjabi history during the Mughal period. As a result, his attempt in the last chapter to place the development of Sikhism and the Sikh-Mughal conflict in a larger historical context falls short. A more complete presentation of the religious issue would have allowed him to more thoroughly integrate the religious with the social, economic, and political themes. He has, however, already made a start. His hypothesis that Sikh discontent and hostility were the result more of socioeconomic change than of religious conflict or political domination, while intriguing, is no more than hinted at.

This volume is a useful corrective to the traditional historiography on the Panjab and a welcome addition to the burgeoning group of regional studies on India under Mughal rule. One only hopes that in the future Singh will find the time to place the Sikh material in the rich and interesting context he has developed here.

STEPHEN P. BLAKE
St. Olaf College

ANNE SALMOND. *Two Worlds: First Meetings between Maori and Europeans, 1642–1772*. Honolulu: University of Hawaii Press. 1991. Pp. 477. $38.00.

Anne Salmond begins with the words of Mohi Tawahi on the contrasting prominence and power of Maori and European histories of the islands that came to be called New Zealand. "Our sayings will sink to the bottom like a stone, but your sayings will float light, like the wood of the whau tree, and always remain to be seen. Am I telling lies?" The words of the Maori

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leader, delivered at Hokianga on February 12, 1840, during deliberations over the Treaty of Waitangi, preface Salmond's text and its purpose (p. 11). The author seeks to rethink the first meetings between Maori and Europeans by reading European narratives of discovery against Maori tribal histories and archaeological findings that temper the myths, misconceptions, and bigotries of popular talk and thought.

Lavishly illustrated and meticulously researched, this book details the encounters between different groups of Maori people and the voyaging parties of Abel Tasman, James Cook, François-Marie de Surville, and Marion du Fresne in the period from 1642 to 1772. Introductory chapters delineate the historical contexts and cosmographies of the two worlds about to meet. A significant contribution of the book is Salmond's careful evaluation of the worth and accuracy of the narratives produced by these voyagers as ethnographies of Maori material culture and history. The accounts from the expeditions of Cook and du Fresne stand out for the quality of their efforts to understand and describe Maori life. The documentation provided by Surville's men, however, "was shaped as much by their own interpretations and behaviour as by those of the local people, and in their accounts of local life they mirrored their own mythologies, which at times they mistook for the truth" (p. 356).

The strength of the book lies in its at times impressive ability to explain the strikingly different receptions given European explorers in various areas of the North and South islands, the identity of key Maori actors left nameless or misnamed, and the sometimes conflicting assessments given by different groups of Europeans of various Maori practices. The death of Marion du Fresne in 1772 takes on new cross-cultural meaning when presented against the tense, hostile atmosphere in the Bay of Islands area caused by the Ngaapuhi tribe's encroachment of Ngaati Pou lands. We learn too that Te Raatuu was a Rongowhakaata chief from Tuuranga-nui and not the great prince of New Zealand Joseph Banks believed him to be. Salmond explains the disparity between British and French assessments of Maori women's sexual behavior in terms of the relatively greater exposure and sensitivity to Polynesian culture gained by the literate members of Cook's crew in their prior extended stay at Tahiti. The Frenchmen's generally negative accounts of Maori women were affected by the strategy of sexual enticement as prelude to ambush employed against them at Tokerau, and the sexually explicit derision and insult they experienced at Paatia-matariki for their refusal of earlier, more subtle offers of sexual hospitality by elite Maori women.

Despite the publisher's advertisement, this book marks not so much a new provocative approach to the history of culture contacts as an example of the ethnographically informed study engendered by the now decade-old conjuncture of history and anthropology in the Pacific. Some readers might take exception to the sweeping generalizations used to characterize precontact Maori society or life in seventeenth-century Europe. "Must have been" is a phrase too often and too quickly used to suggest alternative readings of Maori reactions to European manners and technology. More importantly, Maori voices still struggle to be heard in a book dominated by European sources and discursive strategies. Still, Salmond makes an important contribution to the development of New Zealand and Pacific studies. Her book's legacy may well lie not in the history that it is, but in the future histories that it inspires and helps to make possible.

DAVID HANLON
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VOLKER SCHULT. *Mindoro: A Social History of a Philippine Island in the 20th Century; A Case Study of a Delayed Developmental Process*. Manila: Divine Word. 1991. Pp. 213. $20.00.

This book by Volker Schult—an abbreviated, English-language version of a dissertation written in German—is the first scholarly historical study of Mindoro, the seventh largest island in the Philippine archipelago. Based on extensive archival research, it is a mine of valuable information, but, unfortunately, the book has serious flaws, both stylistic and substantive.

Schult focuses on developments in the twentieth century. He tells us much about changes that took place in Mindoro during the four decades of U.S. rule: the creation of new municipalities, an increase in coconut production, an emerging dependence on the U.S. market, population growth due to in-migration, and the attendant dislocation of the Mangyans, the island's upland peoples. Still, as Schult makes clear, Mindoro was underdeveloped on the eve of World War II. Insufficient funds were spent on infrastructure, with the result that the road system was woefully inadequate. Industry was virtually nonexistent. Furthermore, U.S. rule had little impact on Mindoro's political life: landed families that had dominated local politics in the Spanish era continued to hold power.

After briefly discussing World War II in Mindoro, Schult turns his attention to the post-independence period. In 1950, Mindoro was divided into two provinces, Occidental and Oriental Mindoro. In both, as Schult ably demonstrates, politics changed considerably, as new families with new bases of support (for example, the Medallas in Occidental Mindoro, who received many votes from recent Ilocano settlers) emerged to challenge the old. Both provinces experienced a population explosion. Noteworthy economic changes occurred. But, as before, the develop-

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ment of Mindoro was uneven. Although the island's economy became diversified (due in some measure to World Bank projects initiated in the era of Ferdinand Marcos), living conditions did not improve much and tenancy increased. In addition, policies promoted by the Marcos regime led to deforestation and continued exploitation of the Mangyans.

Here then is Schult's well-researched story. Sadly, there are problems in the telling. The text is riddled with awkward and ungrammatical sentences, punctuation errors, and the like. Quotations from Spanish, German, and Tagalog documents are rendered in the text in the original. (Translations of the German and Tagalog passages appear in the endnotes; the Spanish quotations are never translated.) Many factual details included in other notes are valuable (for example, those about factional conflicts and property distribution) and should properly have been included in the text.

Beyond that, Schult fails, in my view, to place the particularities he describes into a meaningful context. He often refers to Mindoro's "delayed developmental process" but never tells us how the situation in Mindoro compares to that in other parts of the Philippines, or in other parts of the world, for that matter. He writes at length about local politics but does not explain whether the political changes in postwar Mindoro are similar to, or different from, those in the rest of the archipelago. Thus, while Schult has done a fine job of research, he has not presented his findings as effectively as he could.

GLENN ANTHONY MAY
University of Oregon

UNITED STATES

STEPHANIE COONTZ. *The Way We Never Were: American Families and the Nostalgia Trap*. New York: Basic Books. 1992. Pp. viii, 391. $27.00.

In recent years a number of impressive books have sought to place the contemporary crisis of the family into historical perspective, to draw out the implications for public policy, and to translate specialized historical and sociological studies into terms useful for policy makers, journalists, and the educated public. The pioneer in this field was Mary Jo Bane, who in *Here to Stay* (1976) argued that however radically Americans had altered their living arrangements, commitment to marriage, family, and children remained strong, and that public policy should seek to promote sexual equality and equal opportunity for children without undue professional or governmental interference. Arlene Skolnick, in *Embattled Paradise* (1991), maintained that a preoccupation with white, middle-class families of the 1950s had seriously distorted public debate on the family by obscuring long-term trends in divorce, sexual mores and behavior, and family structure, and that a major goal of public policy should be to help families adapt to changing gender roles.

Stephanie Coontz's book, an important addition to this literature, follows naturally out of her earlier study, *Social Origins of Private Life* (1988), in which she offered a chronological account of American family history that tied changes in family structure and function to shifts in the social relations of production. Her new volume is organized around a series of myths and misconceptions that inform popular understanding of the family. One of the popular stereotypes that Coontz seeks to correct is the "golden age" conception of the traditional family as a stable, organic, multigenerational unit in which children derived support from grandparents and other relatives and family members worked harmoniously together; she calls this myth "an ahistorical amalgam of structures, values, and behaviors that never coexisted in the same time and place" (p. 9). She also critically reexamines the idyllic image of the 1950s *Leave It to Beaver* family, describing it as a historical anomaly and media fantasy masking the actual diversity and problems of postwar families. In addition, the book draws on sociological studies to dispute the myth that the disintegration of urban black families is the major cause of poverty among African Americans.

One of Coontz's goals is to offer a historically informed critique of the notion of "traditional family values," defined in terms of household self-sufficiency, parental autonomy, and a clear gender division of labor. She draws on recent scholarship to show that American families have never been economically self-sufficient; that the historical record reveals a wide diversity in family forms and childrearing arrangements; that domestic violence, abuse, and abandonment were common in the past; that mothers have rarely been able to devote their time wholly to their children; and that despite the lip service Americans pay to the notion of family privacy, the country has a long tradition of subjecting "deviant" and poor families to government regulation and oversight.

Coontz's other basic aim is essentially political: to trace the roots of contemporary family problems to changes in the economy, most notably stagnating real wages. Describing the problems of today's families as symptoms of "a larger crisis of social reproduction" (p. 277), she locates the causes of current family ills in the problems of balancing paid work, housework, and childcare; the declining job prospects of lower-income African-American men; the "incomplete" integration of women into the paid workforce; and the commercial exploitation of private life. Her concluding message is that nostalgia for a mythical golden age of stable, intact families should not prevent us from addressing the novel problems of the present.

STEVEN MINTZ
University of Houston

ROBERT N. BELLAH *et al. The Good Society*. Paperback edition. New York: Vintage. 1992. Pp. viii, 347. $13.00.

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This is a book about American institutions: why they are important, how they are failing, and in what ways they should be reconstructed. Robert N. Bellah and his associates argue persuasively that neither scholars, politicians, nor ordinary citizens appreciate that "we live in and through institutions" (p. 256). It is the great achievement of this book to inject consideration of institutions into the conversation about how to reconstruct America. Extending the theme of their earlier book, *Habits of the Heart*, the authors argue that a radical individualism has dominated American culture since the late eighteenth century. Increasingly incompatible with big government and the interdependent world of the twentieth century, individualism has undermined the goals it sought to foster and distracted Americans from the importance of their collective life, which is lived through institutions.

Individualism has distorted American values and institutions by fostering materialism and the hegemony of a market model for all human and social relations. It has denigrated collective activity and spiritual values and distorted democracy. As a result, institutions work badly, the public sphere has atrophied, and Americans remain unfulfilled at work, home, or play. Bellah and his associates illustrate their argument with studies of education, religion, and foreign policy. They draw on secondary literature and interviews with thoughtful, articulate, and dissatisfied institutional participants.

Historians, however, will find serious problems in this book. It is romantic about earlier periods in American history, including both the eighteenth century and the 1950s. It often refers to "Americans," as if all of them think alike. Specialists will find its discussions of specific topics, such as education, superficial, relying on a limited number of secondary works, not always the most current, and not grounded in a grasp of current historiography.

The book's causal arguments are either weak or nonexistent. The authors pin the ascendancy of individualism in American culture on John Locke without exploring how and why it has been transmitted from one generation to another. They do not trace the process through which individualism translated into politics, policy, or social action. They assign individualism, a facet of culture, far more responsibility than it can bear for the dynamic of American history and the shaping of institutions. For some reason, they do not consider the relation between individualism and capitalism, which could account for many of the phenomena they describe and, at the same time, supply the engine of change missing in their account. The authors also jumble time by drawing on social critics throughout the twentieth century, all of whom deplored the excessively individualistic streak in American society, its materialism, and its neglect of institutions. Yet the authors want to argue that the situation they described dates primarily from the 1960s.

This book not only fails to explain how America became what it is but it also lacks an explanation of how it is to become what the authors want. Each major chapter ends with exhortations, statements about what American institutions should become. Yet the authors do not suggest how we are to get from here to there. Surely the task requires more than moral suasion.

MICHAEL B. KATZ
University of Pennsylvania

TERESA AMOTT and JULIE MATTHAEI. *Race, Gender, and Work: A Multicultural Economic History of Women in the United States*. Boston: South End Press. 1991. Pp. xiii, 433. Cloth $40.00, paper $16.00.

Emphasizing the essential and diverse character of women's "works," Teresa Amott and Julie Matthaei offer a unique, truly multicultural economic history of women in the United States. As radical political economists, Amott and Matthaei assume the importance of power and hierarchy in economic history and highlight the exploitation and oppression of women in the process of capitalist development. But they also contend that gender cannot be separated from class and race or ethnicity; the categories are "interconnected, historical processes of domination and subordination" (p. 11). Asserting that "there is no common experience of gender oppression among women" (p. 13), the authors closely examine the relationship of women in six racial and ethnic groups to the economy. The result is a provocative and compelling interpretation of American economic history.

Six chapters separately survey the historical experience of American Indian, Chicana, European-American, African-American, Asian-American, and Puerto Rican women in the United States. The overviews occasionally lose their focus on women and the economy or are overly simplistic or tendentious. But the chapters effectively draw on and synthesize a wide range of secondary works and serve to "accentuate the differences and conflicts among women of different racial-ethnic groups" (p. 28). They together provide the basis for analysis of the relative significance of gender, class, and race/ethnicity over the course of three stages in U.S. economic history.

In the precapitalist period, spanning the colonial era through the mid-nineteenth century, a variety of nonwage labor systems tied a woman's work more closely to that of men of her racial-ethnic group and class than to that of women in other systems and classes. Once families became involved in wage labor, however, they created sexual divisions of labor that were similar across racial-ethnic groups. In the second stage, from 1830 to 1930, men and a large proportion of single women were concentrated in wage work while most married women were engaged in unpaid reproductive work. Since 1980, the majority of married women have moved into wage work while retaining responsibility for unpaid reproductive work. Stressing the ambiguous impact of capital-

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ism, the authors do not conclude that women's lives have uniformly or simply improved. Women as a group have become more economically independent, but the labor market exposes them to discrimination, sexual harassment, and the double day. Although women have gained access over time to new jobs such as clerical work and in the professions, labor market hierarchies based on race and ethnicity as well as gender endure and have been reproduced. Amott and Matthaei also emphasize that despite the convergence of female experience in the labor market, differences among women persist and are particularly apparent in the variety of their demands and strategies for economic change.

There is little original research in this book and much of the argument about change and continuity in the history of women and the economy is familiar. The book, however, contributes substantively and theoretically to the ongoing discussion within feminist theory about the significance of difference. An emphasis on women's agency also distinguishes the volume. Oppression and exploitation are inevitable features of capitalism, Amott and Matthaei contend, but people are not passive victims. Women's individual and collective efforts to resist injustice are especially prominent and effective in the group histories. Despite its limitations, the book's consistent focus on the multicultural dimensions and dynamics of women's history makes it both unique and valuable.

NANCY F. GABIN
Purdue University

ANDREW KULL. *The Color-Blind Constitution*. Cambridge: Harvard University Press. 1992. Pp. x, 301. $35.00.

Until the final chapter, Andrew Kull's history of the idea that the Constitution should be color blind provides an informative and dispassionate analysis of a controversial legal theory. Justice John Marshall Harlan's dissent in *Plessy v. Ferguson* endorsed the phrase and gave the concept a prominent place in constitutional discourse.

Kull finds the origins of the concept in antebellum controversies over school segregation in Boston. His focus on the concept of color blindness illuminates aspects of a familiar story that sometimes are overlooked. So too with his treatment of the congressional debates on the Fourteenth Amendment. Although specialists may be familiar as well with the late-nineteenth-century desegregation cases Kull discusses, the cases deserve attention from the broader audience to which Kull's work appeals.

Kull helpfully distinguishes between an optimistic and a pessimistic vision of the possibilities for legal regulation of race in the United States. Optimists believe that legislatures, supervised by courts requiring that legislatures act reasonably, can promote the interests of racial minorities. Kull shows how this optimistic vision was shared by the otherwise quite opposed Supreme Court majorities in *Plessy v. Ferguson* and *Brown v. Board of Education*.

Pessimists, in contrast, support the idea that the Constitution should be color blind because they lack confidence that either legislatures or courts will do a good job in ruling out policies that actually harm racial minorities. Pessimists believe that both legislatures and courts, sharing their culture's views of what is reasonable, will endorse as "reasonable" policies that reflection shows are harmful. Their solution is to bar government from using race as a basis for action or classification, even when proponents contend that the regulation at issue actually advances minority interests.

In the final chapter Kull turns to contemporary controversies over affirmative action, whose proponents take the optimistic view. Here Kull's analysis slips somewhat. The preceding chapters persuasively establish two propositions. First, proponents of African-American interests have historically taken the pessimistic view in insisting that government completely refrain from using racial classifications; they have advanced the "color-blind Constitution" theory. Second, that theory has almost always been a minority position. Kull shows that it was proposed and rejected in the debates over the Fourteenth Amendment, rejected again in *Plessy*, and not adopted in *Brown*. Only for the decade between 1955 and 1965 was there some reason to think that the "color-blind Constitution" theory had been embedded in constitutional law, and even during that period, Kull shows, the courts were on the whole quite careful not to endorse it.

As a result, it is difficult to contend, as Kull occasionally does, that the Constitution interpreted in light of its history and development actually embodies the "color-blind Constitution" theory. His demonstration that proponents of African-American interests have historically been strongly attracted to the pessimistic view, however, does support a somewhat different, non-originalist justification for the theory. In the end, Kull's history, which is provided in detail, is more interesting than his forays into constitutional interpretation.

MARK TUSHNET
Georgetown University Law Center

NANCY V. BAKER. *Conflicting Loyalties: Law and Politics in the Attorney General's Office, 1789–1990*. Lawrence: University Press of Kansas. 1992. Pp. viii, 248. $25.00.

Nancy V. Baker offers much insight into why the office of U.S. Attorney General has so often been embroiled in controversy in American political history. She traces the problems of the office to the conflicting expectations for an officer who must be loyal both to the law and to the president. The attorneys general serve as both legal advisers and

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policy advisers, and, although Baker notes that these two roles do not necessarily conflict, they do "coexist in an inherent tension" (p. 3). The problem for attorneys general has been to find the appropriate balance.

Baker uses role theory to provide a framework for the historical evidence that she examines. One way for attorneys general to adapt to the competing norms governing their behavior (independence, nonpartisanship, and loyalty) is to emphasize one over the others. She proposes that we consider the roles of the attorney general on a continuum where the independence norm marks one end (the "neutral") and the loyalty norm marks the other (the "advocate").

The "neutral" is characterized as one who interprets and applies the law in an apolitical manner, is a newcomer to partisan politics, is not a close friend to the president, and avoids active involvement in policy disputes. The rule of law is valued over loyalty to the president. The "advocate" is a political activist who views law as a means to particular policy ends, values majoritarian rule, and considers it a responsibility to serve the interests of the president and the electorate.

Baker uses a case study approach to test the validity of her two "ideal types." In explaining the "advocate," she considers the tenures of Roger Taney, Caleb Cushing, Homer Cummings, Frank Murphy, Robert Jackson, Robert F. Kennedy, and Edwin Meese. She also examines the tendency of partisan advocates to become embroiled in scandal. Her consideration of the "neutral" type examines the service of William Wirt, Benjamin Brewster, Harlan Fiske Stone, Edward Levi, and Griffin Bell. Baker notes the appointment of "neutrals" following periods of scandal.

Baker is at her best when providing historical examples of the conflicting tensions faced by attorneys general. She marshals considerable evidence to demonstrate the "structural duality" (p. 4) of the office, drawing extensively on biographical works as well as original documents and interviews. Her book is weakest in its attempts to superimpose role theory and the "ideal types" on this rich historical record. Baker frequently points to the exceptions each case study offers to the type. For example, whereas Griffin Bell considered himself a "neutral," the press and public perceived him as an "advocate" because of his close ties to President Jimmy Carter. John Mitchell breaks from the "advocate" type because he lacked a partisan background. One is left wondering about the worth of the category when the primary examples fit the type so awkwardly. Baker acknowledges that her scheme is "rudimentary" and that the ideal attorney general finds balance between the roles.

This is a well-written and worthwhile study of an important and often misunderstood office. It contributes to our understanding of the inherently political nature of the office.

KATY J. HARRIGER
Wake Forest University

BOB PEPPERMAN TAYLOR. *Our Limits Transgressed: Environmental Political Thought in America.* (American Political Thought.) Lawrence: University Press of Kansas. 1992. Pp. xiii, 184. $25.00.

Henry David Thoreau and Gifford Pinchot have become well-worn symbols for American historians of environmental thought. If Thoreau, through his organicist, romantic vision of harmony and interdependence in nature, embodies a biocentric ethic, then Pinchot reflects an anthropocentric managerial ethos in which nature is valued only in relation to its utilitarian worth. In this valuable survey, which focuses primarily on twentieth-century American environmental thought, Bob Pepperman Taylor too begins with these iconic figures, but he interprets their meaning quite differently from standard accounts. Both Thoreau and Pinchot, Taylor argues, were united in their belief that environmental thought was an integral part of political discourse. Whereas Thoreau looked to nature for a critique of American society, Pinchot regarded conservation of natural resources as essential for achieving distributive justice. It is this political character of environmental thought that has been lost in the recent debates between anthropocentrism and biocentrism within the environmental ethics literature, a debate that has obscured our historical understanding of environmentalism in the United States.

In four succinct chapters, Taylor charts the dissociation of political vision from the pastoral and progressive strands of environmental thought in the twentieth century. Analyzing the neo-Malthusian writings of Paul Ehrlich, Garret Hardin, and others, Taylor shows how fears about overpopulation and scarce resources following World War II severed the connection to liberal democracy found in Pinchot's progressive conservationism and substituted in its place authoritarian management strategies. Unlike the neo-Malthusians, authors such as Roderick Nash, Christopher Stone, and Mark Sagoff maintained a commitment to democratic liberalism but rejected Pinchot's utilitarianism in favor of alternative foundations for environmental values. Taylor contends that in doing so, however, these writers have also discarded Pinchot's commitment to democratic equality.

Other recent environmental theorists have looked to the pastoral tradition of Thoreau in establishing an alternative perspective, but they too have shorn this tradition of its political vision. Taylor demonstrates how John Muir's infusion of spiritual values into nature stripped Thoreau's environmental ethic of its politically reformist goals. Muir's radical transcendentalist ideals have been embraced by adherents of deep ecology, yet this movement, despite its anthropocentric critiques, is heavily imbued with a philosophy centered on self-realization. Biocentric theorists such as Holmes Rolston and J. Baird Callicott have sought to avoid the inherently anthropocentric orien-

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tation of deep ecologists by looking for intrinsic values in nature that transcend human desires and needs. Yet in seeking to derive moral principles from nature based on the science of ecology, they neglect the extent to which science itself is a social activity, open to negotiation and interpretation. In the end, Taylor argues, biocentric theorists have developed a philosophy to defend the preservation and protection of nature, but one that is bereft of dealing with problems facing human society. Taylor concludes with an analysis of the writings of Barry Commoner and Murray Bookchin, arguing that these are promising attempts to restore political vision to American environmental thought. Taylor's central message, that environmental thought is political, provides needed historiographic insight into the history of American environmentalism and offers an important perspective for approaching contemporary environmental problems.

GREGG MITMAN
University of Oklahoma

ROY ROSENZWEIG and ELIZABETH BLACKMAR. *The Park and the People: A History of Central Park.* Ithaca: Cornell University Press. 1992. Pp. xi, 623. $35.00.

One is struck at once by the originality and revisionary power of Roy Rosenzweig and Elizabeth Blackmar's work. The book satisfies all expectations of a standard history; it offers as much of an adulatory chronicle of the nation's most famous public park and its changes through time as we might conceivably desire. But this in itself, achieved through diligent research and admirable narrative skill (which manages to survive excessive repetition and redundancy), is hardly the full story. Rosenzweig and Blackmar aim at nothing less than a complete redefinition of their subject.

What is Central Park? We have been conditioned to believe that the name designates a piece of artfully reconfigured land, the tangible realization of a design by Frederick Law Olmsted and Calvert Vaux (who emerges here from the shadow of his partner). The authors discover another, more complex, nuanced, and vibrant referent. Viewed by itself as a jewel in the careers of its designers and administrators or as a landmark in landscape architecture, isolated from the city that surrounds it, the workers whose physical labor shaped it, and the "cultural public" who used it, the park presents a historical void. The authors reconceive the park as the site not only of labor but also of conflicted meaning, of meanings that have changed in relation to changes in the city. Their Central Park is less a place than a text bristling with social implication. They want to fill the void by recovering the history of labor, use, and conflict. The authors spin a wonderfully vibrant fabric in which threads such as the park's role in inaugurating state intervention in the real estate market are woven together with such matters as the brilliantly reconstructed account of the community of African-American and immigrant settlers displaced by the park. Most often they hit their mark with skill and grace.

With superb concreteness Rosenzweig and Blackmar portray the nexus of city and park as a realm charged with conflict and contradiction. Their cast of competing figures includes gentlemen merchants, real estate speculators, journalists, taxpayers, labor and ethnic leaders, society ladies and middle-class mothers, uptown and downtown residents, gay men, and women athletes. Their Central Park is the site of the city's, and nation's, central contradiction: a potentially egalitarian form (the public park) created by an unequal, class-ridden capitalist society. From the history of controversy and contestation about location, design, administration, accessibility, and permissible uses, the authors derive their leading questions. They ask (p. 10): "Is a park best understood as a designed natural landscape or as a common space shaped by ordinary parkgoers? Who constitutes the public, and how does the public participate in making decisions about the park's management and use? How should the park accommodate the conflicting expectations of different groups of parkgoers and city residents?"

Note that the questions are as much historiographical as political, as much about the method as the subject of the book. The authors present their book as a populist contribution to the continuing struggle over definition and meaning. Rather than a "pure" form set down in the middle of the island by benevolent and authoritative genius, the authors re-create a park shaped by contest. On one hand, there are the wealthy, who founded the park as a public space and behaved at first as if it were theirs alone to enjoy and control, and, on the other hand, there are the heterogeneous ordinary people of the city, who increasingly took the word "public" to include themselves and their recreational needs and desires. Rosenzweig and Blackmar tell the story of a park ultimately authored not by single figures of talent and power but by a collective ensemble, "the people" of the book's title. There is a telos to the story, a process of increasing democratization. In the end even Robert Moses, as Olmsted did earlier, had to acknowledge the superior power of an increasingly inclusive, representative, and activist body of users to define the "public good" of the park.

What does "public good" mean, whoever defines it? The authors make prominent use of conceptual terms—"upper class," "working class," "the people"—that often appear less as precise social markers than as overdetermined interpretations. Regarding "public" and "democratization," the authors' point is that controversies about the park reveal a contested underside; disputes over recreational uses harbored deeper disputes over social control and political power. Yet the key terms never get discussed in their own right. Neither does the class interpretation that underlies the book's argument. Perhaps the method of narrative social history disclaims discussion of

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things "in their own right." But the absence of conceptual or theoretical discourse weakens the argument. This lack is especially telling in regard to the implied telos of progressive democratization as a consequence of wider popular use of the park, as if greater numbers and more heterogeneous uses already constitute or bring about democracy.

The authors conclude that "the greatness of Central Park has more to do with these democratic possibilities than with the artful arrangement of trees, shrubs, bridges, paths, and lawns" (p. 530). Yet is the art of the park, its experiential form, irrelevant to democratic possibility? "Use" preempts the more difficult matter of "experience." Affective and subjective realities of the park, something different from opinion or perception, rarely break into the social narrative (hints appear on pp. 338 and 526). The authors stop where they might well have continued; a substantial theoretical and philosophical literature exists on the role of open-air festival and spectacle in social and political life, and on the role of everyday public occasion in private life. Olmsted especially, whom the authors scorn as an autocratic administrator, has something of interest to say about the public character of the subjective realm. But speculations and ideas as such make only passing appearance in this rigorous social history. Rosenzweig and Blackmar score a major success in winning the history of Central Park away from Olmsted idolaters and preservationists, and in recasting that history in terms relevant to contemporary struggles for urban democracy. But on its own deepest matters the book remains reticent.

ALAN TRACHTENBERG
Yale University

NAOMI W. COHEN. *Jews in Christian America: The Pursuit of Religious Equality.* (Studies in Jewish History.) New York: Oxford University Press. 1992. Pp. viii, 300. $39.95.

Early America assumed itself to be a Christian nation, and that assumption entailed a certain humiliation for Jews and Judaism. In the colonial and early national period this attitude sometimes meant limiting Jews' franchise or compelling them to close their businesses on Sunday. To this day there are dozens of slights based on religion that remind Jewish citizens that they are considered something less than equal by their Christian neighbors. Naomi W. Cohen painstakingly traces the Jewish struggle to erase that stigma from its colonial beginnings to the Supreme Court decision upholding the right of the municipal authorities of Pawtucket, Rhode Island, to erect a crèche on public property (*Lynch v. Donnely* [1984]).

The Pawtucket decision makes it seem as if the struggle for religious equality has reverted back to where it stood three centuries earlier. But in fact in the intervening centuries the Jewish position, which envisaged a wall between church and state, has won general acceptance, at least in the courts. The Pawtucket decision merely indicates that the notion that America was a Christian nation was never quite forgotten. Beneath it all was the assumption by the Protestant establishment that the nation needed a core of Christian beliefs to remain moral and true to itself. Inevitably, that placed Jews outside the consensus and they reacted with a pervasive separationist position.

There were opportunities for Jews to choose a less-confrontational path. A society willing to recognize Judaism's equal worth might have been sufficient and spared them much pain. But something within their own development, perhaps their growing secularization, compelled them to make separationism the heart of a strategy to win religious equality. As a result, they suffered the calumny reserved for those who were considered "godless" in a god-fearing nation. It fell to Leo Pfeffer, counsel for the most secular of the Jewish defense agencies, the American Jewish Congress, to develop the separationist position in the courts. There is something paradoxical in that. The people who so staunchly fought for religious equality had by World War II become indifferent to their own religion. Its most religious elements, the Orthodox Agudath and some of the Hassidic courts, broke the separationist united front to support government aid to religious schools and the display of religious symbols in public space. The separationist consensus was no longer complete. There were a growing number of Jews who wondered whether the recitation of an innocuous prayer in the school or a moment of silence really hurt the Jewish interest.

Those familiar with Cohen's prior research, and all who work in American Jewish history are required to be, know that her narrative style makes few concessions to the casual reader. When she writes legal history, as she does in the second half of this book, the density of the prose requires close attention lest the reader miss the nuances. Those who persevere earn a special reward since this work presents the American Jewish experience in an entirely new light and is in addition a research tour de force. There are, of course, times when the reader sees a better way to present the material. The legal argumentation on "innocuous prayer" or religious symbols in public places would make more sense to the non-legal minded reader if the concept of America's developing civil religion were employed. But such "might have dones" are, after all, the choice of the researcher. In the end this book stands on its own. It finally gives American Jewish historiography a picture of its varied legal and institutional response to being "strangers in the land." In a host nation that above all else reveres the law, that story is indispensable for understanding the American Jewish experience.

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PETER LEVINE. *Ellis Island to Ebbets Field: Sport and the American Jewish Experience.* New York: Oxford University Press. 1992. Pp. xii, 328. $25.00.

There was a popular joke in the 1960s:

"What's the shortest book in the world?"

"Famous Jewish athletes."

Now Peter Levine has given us over three hundred pages of famous Jewish athletes. The data are impressive: between the two world wars, hardly a year went by without a Jewish champion in professional boxing, and Jewish fighters held eighteen titles between 1920 and 1940; during roughly the same period, Jews stood out on college basketball teams (such as the City College of New York) and in the professional American Basketball League; although Jews have not been a large numerical presence in major league baseball, players like Hank Greenberg, Al Rosen, and Sandy Koufax have more than made their mark.

Even more interesting is the tale Levine tells about mass participation of Jews in amateur athletics. Through oral history interviews, personal memoirs, and organization records, he reconstructs a Jewish sporting life characterized by mass participation. Sports, Levine finds, were part of the cultural world that was fostered in the Jewish neighborhoods of large cities by the children of the immigrants. For many of the first generation, however, sports made little sense. A father wrote to Abraham Cahan, editor of the *Jewish Daily Forward*, "what is the point of a crazy game like baseball? The children can get crippled . . . I want my boy to grow up to be a mensh, not a wild American runner. But he cries his head off." The problem was that fully grown men set a bad example: "Here in educated America adults play baseball. They run after a leather ball like children." Jews learned by example, and shortly after the turn of the century, the Young Men's Hebrew Association (sort of a Jewish YMCA) had branches in every major city, and the Jewish press avidly covered Jewish sports.

Dozens of specific examples from Jewish community life make this book interesting. Imagine a semiprofessional basketball team made up of former settlement house boys and high school chums, that played under the sponsorship of South Philadelphia Hebrew Association. Levine writes, "Wearing uniforms that brazenly announced their Jewishness—tops embroidered with the six-pointed Mogen David and the Hebrew letters semech, pey, hey, and aleph—the team, also known for a short time as the 'Wandering Jews' because they had no home court, played against all comers and participated in the Philadelphia League" (p. 63).

Levine concludes his work with some observations on the role of sports in contemporary Jewish identity. Sports countered the stereotype of Jews as overcerebral, even physically soft; toughness on the playing field, some argued, translated into courage in the face of anti-Semitism. Moreover, sports offered a metaphor for the American dream of upward mobility that the largely working-class second generation wanted so badly to believe. Above all, sports were a bridge to America, away from the languages and rituals of the Old Country to the popular culture of the New. Cheering for Jewish professional athletes or participating in Jewish neighborhood clubs reinforced Jewish identity, even as it brought that identity more into conformity with mainstream American norms. In a final note, Levine cautions against any comparison of black and Jewish sporting experience, noting that the history of racism and the transformation of capitalism over the last few decades make the experience of the two groups dissimilar in crucial ways.

Some readers may find this work too detailed, although the piling up of information does help Levine establish his case that sport was an important part of second-generation Jewish cultural life. More fundamentally, by focusing so much on the theme of becoming American while remaining Jewish, Levine tends to slight the range of ideological commitments held in the second generation. The children of immigrants were indeed becoming culturally American in important respects, but one wonders what self-proclaimed atheists, socialists, and communists thought about sports. Perhaps most important, this work suffers from a lack of gender analysis. By this I mean far more than a token "say more about women." Jewish fans and players, at least the ones Levine discusses, were nearly all men. That must change his interpretation. This story cannot be simply about Jewishness, but about how Jewish men of the second generation chose to define Jewish manhood. These criticisms notwithstanding, Levine tells an important but little-known story.

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ARTHUR POWER DUDDEN. *The American Pacific: From the Old China Trade to the Present.* New York: Oxford University Press. 1992. Pp. xx, 314. $27.95.

Arthur Power Dudden has written a lively narrative history of the more than two centuries of American contact with the Pacific region. Although much of the book is concerned with the relations between governments, Dudden also gives ample attention to the many private enterprises that often spearheaded American involvement in the Pacific. In the process, Dudden provides the reader with finely drawn portraits of many of the merchants, missionaries, educators, captains, and generals who ventured out from America. He also improves on previous conceptions of the American movement into the Pacific by including Alaska's settlement into his story. General readers and teachers looking for an accessible introduction to the topic will be well served by this book.

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Students and scholars more familiar with the subject, however, are not likely to find either new information or interpretations. Organizing a subject of this size is an imposing challenge, and authors face difficult choices. In this case, however, it seems that Dudden's choice of format, although useful to the general reader, may have worked against his call to view the Pacific world as a single geopolitical entity. Instead of an integrated study of the simultaneous movement of Americans into different areas of the Pacific, the reader finds separate chapters dealing with Alaska from the eighteenth century to statehood, Hawaii from the eighteenth century to statehood, the Philippines from 1898 to independence in 1946, and so on. As a result, the interplay and interconnection between these discrete topics is often undeveloped. In some cases the author overcomes this deficiency by repeating material from previous chapters, but for the most part it is difficult to acquire a sense of how major changes in international or domestic circumstances affected the American approach to the Pacific world.

The author has taken advantage of some of the recent literature in the field, but at times his selection of secondary works creates problems. For example, Dudden seems to have relied on Stanley Karnow's otherwise fine *In Our Image: America's Empire in the Philippines* (1989) to explain the roles played by Assistant Secretary of the Navy Theodore Roosevelt and President William McKinley in the acquisition of the Philippines. As a result, Dudden, like Karnow, mistakenly reports that McKinley could not locate the Philippines on a map, and he repeats the tale of how Roosevelt allegedly took advantage of his superior's temporary absence from the office to dispatch orders to the American fleet at Manila. In a later chapter on the Vietnam War, Dudden repeats the long-held misconception that the Eisenhower and Kennedy administrations were haunted by the specter of monolithic communism. Recent works by Gordon Chang, John Lewis Gaddis, and others, however, have shown that American understanding of the regional tensions in the communist bloc was more sophisticated than public rhetoric indicated. Reference to this more recent interpretation actually could have reinforced Dudden's thematic structure by presenting the American reaction to Chinese communism as a response to events in the Pacific region rather than as part of a global strategy to defeat a monolithic communist threat. If on occasion Dudden misses a chance to provoke debate or challenge existing interpretations, he nevertheless has produced a highly readable survey that will help students and other interested readers understand the American experience in the Pacific as we await the arrival of the Pacific century.

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PAUL A. RAHE. *Republics Ancient and Modern: Classical Republicanism and the American Revolution.* Chapel Hill: University of North Carolina Press. 1992. Pp. xiv, 1201. $49.95.

For those who dare to hope that the obsession with republicanism as a useful category of analysis has run its course, the arrival of Paul A. Rahe's four-and-a-half pound volume is an ominous portent. As intellectually ambitious as J. G. A. Pocock's *The Machiavellian Moment* (1975), the obvious work to which it might be compared, Rahe's study is also more accessible and engaging than that celebrated book, and wider ranging. Where Pocock observed the ancient republics through the distant lens of their early modern interpreters, Rahe, a classicist, describes what those republics were about; where Pocock treated American republicanism as an epilogue to a story already concluded, Rahe makes the founding of the American "regime" the climax of his account. In these shifts in emphasis lie virtues—and powerful virtues—by which the promises of the so-called republican synthesis may be redeemed and its vices moderated.

For all its ambition, this is also the work of a conservative academic who expects readers either to slight his ideas or to require something akin to a conversion to appreciate their import. Although he acknowledges a debt to Pocock, Quentin Skinner, John Dunn, "and those who have succumbed to their influence," Rahe joins Leo Strauss and his band of "intrepid admirers" in repudiating historicism as an appropriate method for understanding the ideas and intentions of the greatest thinkers (p. xi). Rahe expresses this affinity in various ways: by using such favored terms as regime, statesmanship, and commercial republic; in numerous notes prescribing the proper order in which readers should examine sources; and in two presumptions crucial to his argument. Rahe holds, first, that the break between ancients and moderns was profound and nearly absolute—that Machiavelli and his successors inaugurated ways of conceiving politics and human activity that were fundamentally alien to the ideas and purposes of the Greeks. Second, against the dominant contextualist view of political thinking associated with Pocock and Skinner, Rahe affirms the possibility that within arguments framed *ad captum vulgi loqui*, knowing readers past and present can discover truly original ideas that transcend circumstantial interpretation.

The argument unfolds in three books whose central claims can be briefly summarized. In book one, "The *ancien régime*," Rahe reconstructs the world of the polis through the categories of Aristotelian political science. Here Rahe ruthlessly recounts what the virtues of citizenship required in the militarized world of the Greek republics. In a series of sketches touching on everything from the lust for honor to the suspicion of commerce and philosophy, from the role

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of homoerotic bonds in maintaining military formations to the distrust of technological innovation, Rahe brilliantly reminds us how utterly committed the Greeks were to a politics in which the distribution of honors, education and culture in all their forms, and economic activity were all designed to preserve civic solidarity.

In book two, "New Modes and Orders," Rahe repeatedly emphasizes how radically the ideas of politics and society advanced by the seminal thinkers of the early modern era (Machiavelli, Bacon, Montaigne, Descartes, Hobbes, Harrington, Locke, Hume and other Scottish illuminati, and Montesquieu) diverged from both the Greeks and the Christian fathers who had conspired to bring the classical world to its end. In Rahe's account, these "partisans of humanity" (p. 249) collectively and cumulatively projected to invent a new world in which philosophy could proceed to subvert religious orthodoxy, reason could be employed to improve the material lot of man, and the purpose of politics could be transformed from the search for illusory moral virtues into a candid effort to manage human weaknesses through the architecture of government and the release of productivity. Rahe makes Locke both the decisive figure in this progression and the commanding inspiration for the American revolutionaries whose achievements are the subject of book three, "Inventions of Prudence."

In this final section, Rahe examines the varieties of republicanism associated with Hamilton, Jefferson, and Madison. To regard these three founders as classical republicans as that term is now used, he insists, misses the crucial point. Their new science of politics was the legacy of the enlightened thinkers who had broken so decisively with the ancients. They sought neither to promote radical civic virtue nor to inspire the lust for honor, but rather to preserve the possibility of self-government in a commercial republic dedicated to fostering the comforts of economic betterment. The partisanship that their rival visions of economic development inspired illuminated the distance between the politics of the American republic and that of the Greek polis. It also provoked competing constitutional theories that impaired the capacity of later generations to agree on those "loved things held in common" (the phrase is Augustine's) that the Greeks understood were required to provide the *politeuma* of the *politeía* with the *paideía* to sustain the *pólis*. The fatal errors were those of Madison and Jefferson: conceding too much to their slaveholder constituents, they undermined the true principles of the Declaration of Independence that were and must ever remain the source of American politics.

Some of the reservations to be voiced about this book are predictably conventional. As a work of historical explanation, it self-consciously asserts a view of individual agency that has nearly disappeared from contemporary scholarship, and which even an old-fashioned historian like myself—someone perfectly content to argue that it mattered a great deal (for example) that Madison prepared the Virgina Plan—finds difficult to credit. Modernity in both its post-Machiavellian and post-Madisonian forms came about, Rahe implies, because great men either willed it so or failed to will it sufficiently, and not because a complex interplay among the *fortuna* of events, the *longue durée* of cultural adaptation, and outbursts of intellectual creativity conspired to drive the process of history. His treatment of the new American science of politics is a case in point. Unlike Bernard Bailyn, Gordon Wood, and lesser acolytes who explain American republicanism as a product of a century and a half of social and political development, the hothouse environment of revolutionary agitation, and the creativity of great and lesser minds, for Rahe it is essentially Publius *et praeterea nihil.*

Rahe's study, however, is less a work of explanation or even revision in the usual historical sense than of interpretation and ultimately exhortation. Rahe, too, has a version of the Pocockian Machiavellian moment: it occurs when modern republics that choose to pursue happiness rather than solidarity confront martial challenges that threaten their existence—as they did when Abraham Lincoln (the Republican who eclipses in moral stature and statesmanship even the three founding republicans) entered the presidency in 1861 and when Winston Churchill was called to power four-score-less-one years later. At such times, Rahe argues, it is not enough to understand the origins of republicanism if one cannot profess why one should love it. The moral is a familiar one to those who have read those disciples of Leo Strauss to whom Rahe seems most indebted: Harvey C. Mansfield, Jr., Martin Diamond, and Harry Jaffa. It also suggests why one should be cautious about treating this book as a work of history in any ordinary sense.

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JON PAHL. *Paradox Lost: Free Will and Political Liberty in American Culture, 1630–1760.* (New Studies in American Intellectual and Cultural History.) Baltimore: Johns Hopkins University Press. 1992. Pp. xvi, 234. $35.00.

This study is a return to old-line high intellectual history. Jon Pahl states unapologetically that he can describe the debate over free will in the life of the early American mind and discern its impact on political culture. After an examination of over two hundred separate colonial publications, he is "convinced that the debates over *freedom* and *providence* and other such terms defined the boundaries of thought and action in early America as fully as military boundaries defined geographical boundaries" (p. xii). The lost paradox, Pahl claims, is the ability to choose freely while simultaneously being directed by Providence. Because of Calvinist assumptions about predestina-

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tion, seventeenth-century settlers were caught in a tautology. How could one demonstrate moral responsibility, that is, act freely, if his or her actions were preordained by God? In the course of debating the issues raised by this contradiction, Calvinists came to share with Anglicans a middle ground with respect to the doctrine of free will.

To prove his thesis, Pahl sees a five-part typology and uses textual analysis of primarily colonial publications. Beginning with the Antinomian affair, Pahl sees the response to John Cotton and Anne Hutchinson as the rejection of two, not one, extremist typologies. Pahl challenges the conventional wisdom of seeing Cotton and Hutchinson as facets of Antinomianism and poses them as two extremes. Cotton is the absolute determinist whose sense of grace is extrinsic and divinely imputed and thus has no place for free will; Hutchinson is the indeterminist where grace is intrinsic and immanent and calls for an absolutely free will. Recoiling from these two extremes, seventeenth-century Calvinists responded with a middle ground exemplified by Thomas Shepard. Rejecting the anarchist inclinations of Hutchinson and the determinism of Cotton, this middle ground kept an externally imputed grace while providing some latitude for the exercise of free will.

Pahl's argument takes this Calvinist middle ground and considers the Anglican and Arminian criticism of it during the eighteenth century. Using Benjamin Franklin, Cotton Mather, Charles Chauncy, Jonathan Edwards, and Jonathan Dickinson, among others, Pahl sets the climactic debate between a Calvinist aristocracy of grace and an anti-Calvinist democracy of law. The typologies are derived from respective Calvinist and Arminian roots. Calvinism limited the exercise of free will and moral behavior to those who had been saved: the aristocracy of grace. Arminianism, however, rested free will and moral action on the notion that redemption was available to the meritorious: the democracy of law. Trying to avoid the limitations on human will posed by predestination, Edwards developed a consistent cosmology that allowed for choice under God's governance and thereby replicated a position held by Dickinson as he coped with the Calvinist criticism that Arminianism made salvation too dependent on the will. Once Edwards made this concession, Pahl argues, he assumed the equivalence of the aristocracy of grace and the democracy of law. When both Edwards and Dickinson moved the question of free will to a secondary level of discussion, presuming that free will is possible after presupposing the presence of God's grace, they reached a common ground for discourse that led to the fifth and final typology, a public theology stressing republican virtue.

In this final synthesis, Pahl sees the religious roots of early American nation-building. Few would argue with the proposition that the republic had a theological foundation, but many might legitimately ask two questions. First, how would Pahl deal with the extensive literature, like that by Philip Gura and David Lovejoy, which sees the assimilation of radical dissenting groups like Antinomians as examples of the Americanization and democratization of Puritanism? Second, how does his description of seventeenth and eighteenth-century thought apply outside the areas of Calvinist hegemony?

Pahl engages the debate over free will in a controversial manner, and his thesis is bold and ambitious. The willingness to rearrange Cotton's intellectual position relative to Hutchinson is evidence of his daring and imagination. But the book's limited conceptual and data base cannot sustain the claims that the discussion about free will speaks to the larger issue of political liberty within American culture.

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ANN FAIRFAX WITHINGTON. *Toward a More Perfect Union: Virtue and the Formation of American Republics.* New York: Oxford University Press. 1991. Pp. xvi, 264. $29.95.

This complex and adventurous book explores the cultural construction of republican consensus in the final years of American revolutionary crisis. Ann Fairfax Withington's point of departure is the Articles of Association, specifically those clauses of the eighth article which banned horse racing, cockfighting, the theater, and expensive funeral attire. These prohibitions, Withington argues, framed a moral code that linked private behavior to the public purpose of forging an American national character, "a character grounded in virtue" (p. 16). She concludes that Americans were psychologically prepared for republicanism before they actually became republicans in 1776. Her methods—extended interpretations of text, symbol, and ritual—bring the new cultural history to bear on the American Revolution.

Withington counterpoises two constructs explaining American rhetoric on the brink of revolution: an "ascetic morality of discipline" and an "aesthetic morality of perception" (p. 12). The latter, rooted in the Scottish Common Sense philosophy, is given short shrift. Appeals to moral sentiment were confined to addresses to Britain and other colonies: the Association's language, aimed at disparate Americans, announced Puritan proscriptions to reinforce an inclusive communal solidarity. Three chapters on "the stage and the scaffold" argue the importance of ascetic rather than aesthetic morality. Long the subject of moral condemnation, the theater was read as a source of moral corruption rather than a means of moral instruction. Comedies, subtle and ambiguous, undermined justice, while tragedies turned the mind to the specific crimes of state-destroying heroic figures. Rejecting the commercial theater's traffic in sordid detail, the revolutionary generation drew on

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the execution for its political dramaturgy. In a fascinating analogy, Withington compares the self-denial of the repentant condemned criminal with that of the ideal republican statesman, shunning heroics in the common cause.

The culture of death becomes one of the book's central themes, developed in a long chapter on New England funeral rituals. Increasingly the focus of corporate identity, funeral expenditures were, from the Great Awakening, condemned as divisive and extravagant. Withington provides convincing evidence that, from the mid–1760s, New Englanders curtailed their purchase of mourning clothes, rings, and gloves. A sixth chapter draws together the implications of these rambling excursions in an analysis of the political mummery of the Stamp Act crisis.

Funerals for Liberty and the Stamp Act, executions of British ministers and local distributors, all acted out in theatrical parody, drew on a host of associations and implications running through eighteenth-century popular culture. A similar structure frames the final two chapters. The first provides a rare description of eighteenth-century cockfighting as well as a discussion of the rise of horse racing from the 1750s: like theater and mourning attire, both were "species" of British corruption. Finally, in her most brilliant essay, Withington explores the code of animal monstrosities to unravel the metaphor linking geese and Tories, realized in the tar and feathers applied by the committees enforcing the Association.

This is a wonderful book, exploiting materials and problems rarely considered in the same frame as the origins of the revolution. There are, however, a number of problems. First, there is the relentlessly homogeneous mode of analysis. With the exception of New England funerals, the book glides above the social and regional diversity of eighteenth-century America, often seeming to assume that the seaport towns and wealthier plantation districts stood for the whole. Throughout, one wonders how a moral code so obviously in tune with a spartan New England was received in regions of less-than-Puritanical tradition. This question of reception leads to a second problem. In large sections of this book the voices of contemporaries outside the printed page are virtually missing. Withington's analysis of the stage is drawn entirely from the colonial press and her description of the cockfight from British sources; the only fleeting private commentary comes from Josiah Quincy's journal. Thus, there are questions about the regional impact and the personal impact of the moral code that Withington invests with so much significance. One wonders whether these themes really can support the entire weight of the psychological construction of American republicanism. As Withington admits, Congress "skirted the periphery of daily life" (p. 14) in framing the moral program in the eighth article, but nonimportation and nonconsumption, exhortations to frugality, industry, and the raising of sheep for wool—the stuff of the greater part of the Articles—went directly to the heart of that daily life, especially for women. It was here, many would argue, that the ascetic morality of the Association came to bear on the individual, the household, and the ordinary community.

This distinction between broad social experience and complex cultural construction may well define an interpretive fault line in our understanding of the popular engagement with republicanism in the opening years of the revolution. If so, Withington may be credited with helping to shape the debate. An exuberant romp through known and unknown corridors of eighteenth-century culture and politics, this book will entertain and challenge all historians of revolutionary America.

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AVIHU ZAKAI. *Exile and Kingdom: History and Apocalypse in the Puritan Migration to America.* (Cambridge Studies in Early Modern British History.) New York: Cambridge University Press. 1992. Pp. x, 264. $44.95.

As we are told in the dust jacket blurb, "this book explores the ideological origins of the Puritan migration to and experience in America, and shows how Puritans believed that their removal to New England fulfilled prophetic apocalyptic and eschatological visions." Avihu Zakai suggests that these visions may be subdivided into categories of "sacred space and time." His book's seven chapters each begin with the phrase "The creation of," indicating that the Puritans themselves appropriated previous traditions of eschatology and apocalypse in "creating" a new sense of meaning in, for example, "The Creation of Sacred Time" (chap. 1), or "The Creation of Sacred Space" (chaps. 2, 3, and 4), "The Creation of Sacred Errand" (chap. 5), "The Creation of a Sacred Christian Society" (chap. 6), and "The Creation of a Holy Christian Commonwealth" (chap. 7). The strength of the book is its organization of eschatology and apocalyptic vision into a framework that is clearly defined and presented. Its weakness lies in the same organizational structure, that is, the oversimplification of an extremely complex problem of appropriation (by the Puritans of existing traditions of eschatology and apocalypse), a topic that is barely touched on in this exegesis of selected Puritan texts, used well by the author to define his organization and to defend his argument.

The topic itself has been well worked over in the past two decades, in books like Ernest Tuveson's *Millennium and Utopia* and *Redeemer Nation* (1972, 1968), Jaroslav Pelikan's *The Mystery of Continuity: Time and History, Memory, and Eternity in the Thought of St. Augustine* (1986), Peter Toon's *Puritans, the Millennium, and the Future of Israel: Puritan Eschatology from 1600–1660* (1970), Sacvan Bercovitch's *The Puritan Origins of the American Self* and *The American Jeremiad*

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(1975, 1978), and my own *The Language of Canaan: Metaphor and Symbol in New England from the Puritans to the Transcendentalists* (1980).

Although the study is clearly written and focuses on a specific historical application of eschatology and apocalypse, it adds little to the previously understood systems of thought that produced these cultural phenomena, and occasionally Zakai provides highly speculative connective tissue to link events to sermons or other expressions of eschatology. There is a carelessness in this methodology, and while it is entirely possible that some of these events transpired as described by Zakai, it is unlikely that the exact connections he infers could ever be proved from textual evidence. For example, in introducing a full exegesis of John Cotton's 1630 sermon, *God's Promise to His Plantation*, the author states that "In March 1630 the Winthrop fleet was gathering at the port of Southampton ready to sail to New England. An almost year-long preparation had come to an end, both in terms of ideological debates and actual material arrangements. Now the eyes of the Puritans aboard the ships were directed toward the holy mission they had dedicated themselves to accomplish in the wilderness of America. They had already made their choice to join in body and spirit this Errand into the Wilderness. But aboard the ships at Southampton, the Puritan emigrants could only have had a dim vision of New England, and obviously many fears lurked in their hearts concerning their wilderness-exile in America . . . At this moment in his life John Cotton was not contemplating a removal to New England. Yet, because he was so familiar with the Puritan mission to America, it was no mere accident that the Puritans who were ready to sail to the New World chose Cotton to preach the farewell sermon. And in his sermon Cotton did not waste the great solemnity of that occasion, wonderfully depicting the Puritan Errand as based on God's prophetic promise to his chosen people that he would lead them to the Land of Promise" (p. 167). The book alternates between gross generalizations such as this and clearly researched exegetical presentations of documents. But the historical record is incomplete, since the generalizations are not well documented and the argument is a well-rehearsed cliché.

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IRMA CORCORAN. *Thomas Holme, 1624–1695: Surveyor General of Pennsylvania.* (Memoirs of the American Philosophical Society, number 200.) Philadelphia: The Society. 1992. Pp. xviii, 317.

The Thomas Holme who emerges from the pages of Irma Corcoran's biography was a vigorous man of action. Born into a middle-class English family, he turned soldier at eighteen and reached the rank of captain in Oliver Cromwell's forces. Having helped conquer Ireland, he worked on the survey of Irish lands and acquired valuable Irish land holdings. Despite his military background, Holme became a leading Irish Quaker. William Penn, who met Holme in the 1650s, tapped Holme as the first surveyor general of Pennsylvania. Holme held political posts in early Pennsylvania and also rendered Penn significant service as a negotiator with Native Americans and Lord Baltimore. But Holme's place in history, as Corcoran emphasizes, rests on his plan for Philadelphia (the often reproduced *Portraiture of the City of Philadelphia*, here given between pp. 116–17) and his 1687 map of the improved part of Pennsylvania (sections appear in the text and the full map is reproduced as the endpapers). The map documents land ownership based on surveys Holme had done and ordered done.

Having noted that in his Ireland period Holme "was not blind to a bargain" (p. 46), Corcoran demonstrates that, as surveyor general, he took special care of himself and his family. Still, Corcoran thought of calling the biography "The Governor's Man," and she emphasizes that Penn rightly described Holme as "my trusty and well-beloved Friend" (p. 284). In assessing how Holme dealt with possible failures by Quakers, Corcoran maintains he had "a tendency toward mercy" and was "prone to reach charitable conclusions" (pp. 70, 144). The author adopts the same approach when judging Holme's own weaknesses. She does not, however, inflate the importance of her subject. Corcoran clearly shows that, aside from his survey work, Holme was not a commanding figure in Pennsylvania's early years.

Corcoran's volume, which thankfully has the notes at the bottom of the page, offers a life-and-times study that also contains ample measures of genealogical analysis. She extensively covers topics—such as Cromwell's military campaign in Ireland, the creation of government in Pennsylvania, and various land ownership battles—without tying chunks of the material directly to Holme. More space is devoted to detailed analysis of the land holdings depicted on the map of 1687 than seems necessary even in the biography of a surveyor general, and the work as a whole contains heavy doses of antiquarianism (for example, pp. 39, 127–28, 184n). In addition, Corcoran advances many "may have" or "must have" observations. Nevertheless, a comparison of the speculative nature of the *Dictionary of American Biography* entry on Holme with the solid factual material Corcoran offers forcefully illustrates the thorough nature of her search for information on Holme and his family. Indeed, Corcoran's study of Holme, which is rooted in years of painstaking research in an impressive range of documents and archives, provides the definitive biography of Holme's career and family connections.

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GEORGE FENWICK JONES. *The Georgia Dutch: From the Rhine and Danube to the Savannah, 1733–1783*. Athens: University of Georgia Press. 1992. Pp. xi, 364. $45.00.

This is an unabashedly old-fashioned book, whose author understands his duty as historian to "discover, gather, verify, and consolidate the evidence and let the reader draw his own conclusions" (p. ix). George Fenwick Jones's goal is the "chronicle, not analysis" (p. ix) of German-speaking persons in Georgia for historians and genealogists. Although the focus is on Germans in early Georgia, the sweep of the story is large, beginning with the description of the ancestral homelands that span the Roman empire, *Völkerwanderung*, Christianization, Reformation and Counter Reformation, and encompassing the Palatines, Salzburgers, Swiss, and Wurttembergers who made up the chief components of the Dutch (that is, *Deutsch*, or German) settlers in the southernmost colony.

The organization of the book is chronological (1733–83) in the depiction of the settlements of Ebenezer, Acton, Vernonburg, and others that included German inhabitants, as well as topical ("Manna from Heaven and Earth"; "Health, Medicine, and Daily Life"; "Indians, Slaves, and Soldiers"). This structure attests to the complexity of the overall story, but it results in a rather disjointed and sometimes repetitive narrative. The strength of the book is the wealth of material culled from primary sources. Genealogists will be well served with the extensive mention of names (stripped of confusing and misleading anglicization), their documentation in the endnotes, and their meticulous inclusion in the index. General readers will also benefit from the contemporary and modern maps (albeit no modern one of the settlements in Georgia) and several portraits (but, unfortunately no list of illustrations).

Historians, too, will find much useful information, but any search for particular data is likely to be frustrating because systematic access through the index alone is not very efficient. Several tables summarizing, for example, how many immigrants arrived; from where; where they settled; for how long; who they married, and how many children (as well as servants and slaves) were part of their households; how they made a living; and whether, how, and how fast they accumulated wealth would have enabled other researchers to draw more readily on data that are particularly intriguing in comparison to settlements of German-speaking colonists outside of Georgia. In this context, the choice of Modern Language Association–style annotation is unfortunate, because its brief and space-saving format discourages notes that point the reader to relevant secondary literature (including Jones's own lifelong work, which focuses on a number of issues, persons, and events in greater detail published elsewhere).

Although readers are left to draw their own conclusions on purpose, the lack of interpretation limits the usefulness of the book for those whose interests are rooted in social history and who engage in the debates centered on the development of local and ethnic communities in eighteenth-century America. They will find it difficult to connect Jones's findings to the analyses discussed and questions posed in the secondary literature because the author is solely focused on the collection, consolidation, and presentation of the evidence and seemingly unaware of the highly selective nature of this approach. For example, the transition from servants to slaves had a profound affect on the Georgia Dutch and the ways in which they organized their lives and plantations, yet only the voices of those who appear prominently in the manuscript sources are recorded and without indication of how representative or unique their experiences were. The isolation in which the German-speaking settlers in early Georgia are portrayed renders a narrow perspective of this ethnically and religiously distinct group of Continental Europeans in the American South.

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JOHN SEELYE. *Beautiful Machine: Rivers and the Republican Plan, 1755–1825*. New York: Oxford University Press. 1991. Pp. xii, 430. $35.00.

Aligning himself with the "myth and symbol" school in American studies, John Seelye traces the neoclassical sensibility of the early national period through the rhetorical figure of the river in both literature and historical events. A dense survey of memoirs, maps, prophetic utterances, travel narratives, fiction, and civil engineering plans, the study begins with George Washington's dreams about the Potomac River as a gateway to the West and ends with the construction of the Erie Canal as the geopolitical project that "had a massive impact on the future course of the United States" (p. 383). Seelye argues that instead of uniting the nation under the banner of the greatest "internal improvement" of the first quarter of the century, the Erie Canal shifted the control of the West to the northern states and prefigured the conflict ahead. To Seelye, the Erie Canal became a particular type of national symbol. "The Constitution was the creation of the Founding Fathers and the Canal was the work of the Sons, the latter a mechanism designed to improve the workings of the former, conceived as a great Republican machine" (p. 328). But, despite the endless celebrations, processions, and parades proclaiming its importance to national union, the Erie Canal, like the Constitution, merely ended in clarifying sectional differences.

The project of the "Sons" was, however, merely one in a long series of dreams about empire and expansion that are continually fraught with the shadows of secession and revolt. Seelye's book is more than anything else a compendium of geopolitical visions

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uttered along the banks of rivers as diverse as the Hudson, the Missouri, the Ohio, the Mississippi, and the Columbia. Weaving together a wealth of material, Seelye finds numerous examples of "two-sided rivers," demarcating north and south, east and west, savagery and civilization, or innocence and decay. Thomas Pownall's sketches of the Hudson portray both "the sheer power" of the "American spectacle," a magnet for newly arriving settlers, and a line between the east and west, settlement and savagery. Other travels would alter this vision, finding the east bank of rivers a continuation of Old World decay, and the west bank the landscape of promise and innocence.

The representations of the rivers create narratives of greed and sectional differences as much as they do the future grandeur of the emerging republican nation. Working with a series of tight comparisons and contrasts, Seelye places, for example, Gilbert Imlay next to Charles Brockden Brown, Zebulon Pike next to Meriwether Lewis and George Rogers Clark. When discussing the Ohio Valley, John Filson is juxtaposed with Reverend Manasseh Cutler to create a landscape of familiar sectional disputes. "Where Filson imposes a quasi-romantic aspect on both Boone and the Kentucky landscape, Cutler retains the traditional New England metaphor of a wilderness garden populated by a chosen tribe" (p. 86). Whether in what Seelye calls "events as literature" or "literature as event," the geopolitical vision of American rivers is deeply conflicted, one in which regeneration and doom are intricately mixed.

In some ways, the fine thesis of the study is frequently submerged beneath its survey quality. Its ambitiousness to include pithy anecdotes about innumerable figures such as Robert Fulton, Albert Gallatin, Elkanah Watson, or De Witt Clinton often waylays the sense of how the rivers as a whole structured the geopolitics of the early national period. The book's movement toward the Erie Canal in a sense comes too late, after a web of quotes and illustrations that lose, at times, their argumentative focus. Carefully researched and written, Seelye's book offers a needed angle on the obsession of early Americans to prophesize their future without the ability to see how they planted the seeds of dissolution. The ironies are sketched, even if the argument as a whole is often elusive.

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J. RITCHIE GARRISON. *Landscape and Material Life in Franklin County, Massachusetts, 1770–1860*. Knoxville: University of Tennessee Press. 1991. Pp. xix, 314. $42.50.

This book is the latest addition to a growing body of scholarship on rural society in New England from the late eighteenth century through the first half of the nineteenth century, a period that witnessed the extension of the market economy and the commercialization of agricultural production. Whereas the bulk of recent scholarship focuses on the social and economic aspects of this "great transformation" in the countryside, J. Ritchie Garrison concentrates on changes in the landscape and material culture of Franklin County in northwestern Massachusetts. Although this book uses material culture to illustrate larger social and economic trends, Garrison's analysis also adds new dimensions to our understanding of those processes and offers a fresh and valuable perspective on this important historical period.

After an initial introduction and an overview of Franklin County's landscape during the colonial period, Garrison analyzes the dynamics of agricultural change in both the lowland townships and the upland areas, which exhibited different but interrelated patterns of development. In subsequent chapters he focuses more particularly on changes in individual farmsteads, dwellings in both the village and the countryside, and the physical impact of an emerging industrial order on the communities in the county. Although Garrison sees the period under study as one of transformation, his focus on material culture and landscape has made him sensitive to continuity as well as to change, and to the variability of individual circumstances. As a result, he deals with the transformation of the Franklin County landscape as a "multilayered" process rather than as a "dramatic shift in world view." Throughout the book, Garrison augments his analysis with a series of excellent maps, photographs, and drawings, many of which he produced himself.

A good illustration of Garrison's methodology and his thematic focus is his discussion of fences, which "signified human power to manage the environment" (p. 118). In contrast to back dooryards, which were places of work, front dooryards were places for public display of status, and beginning in the late eighteenth century local elites began to enclose them with ornamental sawn pickets nailed into milled rails in precise rows and painted white to stand out against the surrounding greenery—altogether a higher level of expense and artifice than the more common split rails or board fencing. By the 1830s, however, most front yards in the villages were enclosed by such fences, reflecting more generally improved economic fortunes as well as "a new level of self-consciousness about the landscape" and "a desire to engross the land in ways that excluded others" (p. 117).

Similar changes marked the specialization in commercial crops as well as the retreat from the common fields and nucleated villages that characterized the older lowland townships. Fence types varied according to need and situation: board fences protected gardens and orchards from small animals, post-and-rail fences enclosed valuable tillage lands, and stone fences allowed farmers to simultaneously demarcate their fields and clear them of unwanted stones. In contrast to the picture-postcard image of carefully

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laid stone walls, however, Garrison notes that most stone fences were simply piles of rocks at the edge of the field. Although such fences may not make good neighbors, to paraphrase the old adage, they certainly make fine history.

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PATRICIA TYSON STROUD. *Thomas Say: New World Naturalist.* Philadelphia: University of Pennsylvania Press. 1992. Pp. xv, 340. $24.95.

Patricia Tyson Stroud presents a straightforward, chronologically organized biography of one of America's pioneer naturalists. Thomas Say set a standard for the transition from nature reportage in America to natural science. To trace his progress is to be introduced to the intellectual and artistic establishment of Philadelphia, the "Athens of America" (p. 28), during the first decades of the eighteenth century, and to the patriotic tenor of natural history following the War of 1812. Say and others believed Americans should name and describe their own flora and fauna rather than cede the authority to English and French virtuosi.

Say greatly expanded his horizons and practical field experience in his early thirties by signing on as naturalist with Major Stephen H. Long's expeditions into the Missouri and the upper-Mississippi watersheds (1819–20, 1823). The particulars of lost bundles of specimens, of stolen scientific instruments, and of all-too-human colleagues put into perspective the achievements of Say in the field and later in Philadelphia as he described his specimens and compared them with those in the literature. Say deserves more recognition than he has gotten, and Stroud strives to bring him and his work alive. Say the scientist dominates the middle portion of Stroud's book; Say the man comes to the fore in the final 100 pages.

On the way to Mexico with his patron, William Maclure, in 1826, Say stopped at New Harmony, Indiana, a community of social visionaries, educational reformers, artists, and naturalists. There he wed Lucy Sistare, who became his partner in publishing by engraving and tinting illustrations of shells for his *American Conchology* (1830, 1838). Stroud details the couple's struggles and joys, the bizarre clashes of personalities, and the hurdles to scientific productivity their residence so far from the center of things entailed.

Over fifty illustrations, many of them portraits by the Peales (Charles Wilson, Raphael, and Titian Ramsay), enrich Stroud's biography, especially the sketches and watercolors by Karl Bodmer. There are also small errors; Ft. Snelling is mislocated as being in St. Paul, Minnesota (p. 141), and the prairie dog is misglossed as a marmot (p. 119). And whereas in one place (p. 121) Stroud erroneously claims that no one had written on American insects before Say did, she reveals later that John Bartam and Mark Catesby had (p. 161). These mistakes an editor should have caught.

More disturbing is Stroud's willingness to attribute psychological motives where no documentation sustains their plausibility: she asserts Say's "unconscious transference of filial duty" (p. 24), his "feelings of insecurity" (p. 41), and that his "self-mastery" was the "object of his self-denial" (p. 100). Stroud's command of primary sources is fine, indeed sweeping; but her grasp of symbolic significance is less firm. For example, she takes the lumping of Indians with other animals in early natural history as a sign only that zoology was "so all-inclusive in the early nineteenth century" (p. 78). That cultural construction antedates Say's time and stretches beyond well into twentieth-century museological conventions.

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ADAM JAY HIRSCH. *The Rise of the Penitentiary: Prisons and Punishment in Early America.* (Yale Historical Publications.) New Haven: Yale University Press. 1992. Pp. xvi, 243. $30.00.

Adam Jay Hirsch's book offers a persuasive new interpretation of the origins of the U.S. prison system. Dense with detail, it is nonetheless highly readable, for Hirsch has relegated technical discussions to footnotes and writes in a style that is at once crystalline and witty.

Hirsch combines economic, institutional, intellectual, legal, and social history to investigate the emergence of the penitentiary and its cultural meanings. He focuses on Massachusetts, the state that led the penitentiary movement by turning the Castle Island fortress into a prison in 1785. This focus provides a much-needed corrective to accounts that locate the movement's origins in the Jacksonian period and the celebrated debate between advocates of the Pennsylvania and New York ("Auburn") systems of incarceration. This is one key respect in which Hirsch's analysis differs from that of David Rothman, whose *The Discovery of the Asylum* (1971) has been the standard work on the subject for two decades.

Hirsch's thesis contrasts with Rothman's in four ways. First, Hirsch argues that Americans' ideology of punishment "did not go through two distinct stages, the first emphasizing deterrence and the second rehabilitation" (p. xiv). Even colonial Americans aimed at rehabilitation. Hirsch usefully distinguishes, however, between their "rehabituative" approach to punishment and the Jacksonians' goal of "reclamation"; the latter, aimed at deeper, personality changes, formed the basis for the penitentiary. Second, Hirsch contends that the ideological roots of incarceration lay not in eighteenth-century Italian rationalism nor (as Rothman argued) in a nineteenth-

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century American social crisis but rather in sixteenth-century English thinking about rehabilitation and in Puritan theology.

Hirsch shows, in his third difference from Rothman, that Americans embraced the concept of incarceration as a punishment well before the Jacksonian period. In fact, carceral solutions to crime were seriously considered and in some cases endorsed before the revolutionary war. "Far from precipitating its rise, the Revolution may actually have delayed the penitentiary for a generation" (p. 86). Differing again with Rothman, Hirsch maintains that few Americans other than Louis Dwight expected the penitentiary to serve as a model for other social institutions. "The vast majority of Jacksonian advocates looked narrowly upon the penitentiary as an instrument of crime control" (p. 67).

In part 2, Hirsch explores the relationship of the penitentiary to slavery. He devotes one chapter to identifying points of congruence between the two institutions, investigating how proponents of incarceration reconciled this position with their opposition to slavery. In his chapter on economic exploitation, Hirsch concludes that, despite the parallels between prison labor and slavery, "Northern captive labor programs were designed to sustain convicts, not to capitalize on their plight" (p. 97).

Given the richness of this ambitious and provocative book, it seems churlish to point to minor flaws. Yet I do wish that Hirsch would have avoided using "prison" and "jail" synonymously (p. 6), an imprecise and confusing usage. Although his study includes over one hundred pages of footnotes and "A Note on the Sources," it would be even more useful to scholars if it listed sources alphabetically. I would have liked to know whether William Penn influenced the adoption of carceral solutions and whether conditions for women convicts differed from those of men. But Hirsch did not set out to write a comprehensive history of the origins and early development of the penitentiary. His goal was to raise new issues and to turn the field in new directions, and in this he succeeds admirably. His book will serve as the definitive work on the rise of the penitentiary for some time to come.

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JEFFREY MEYERS. *Edgar Allan Poe: His Life and Legacy.* New York: Charles Scribner's Sons. 1992. Pp. xii, 348. $30.00.

The torments Edgar Allan Poe inflicted on himself exceeded what he did to his characters. Plagued by poverty, he went out of his way to alienate a rich stepfather; devoted to his child bride, he failed to extricate her from the squalid conditions that contributed to her death at twenty-six; desirous of female companionship (and financial assistance), he subsequently courted various women, some simultaneously and none successfully; desperate for recognition, he denounced the reigning literary figures of his day; hopeful that his work would someday validate his life, he named as his literary executor one of his bitterest enemies. If ever there was a writer whose art can be said to issue from the substance of his life, it was Poe, whose exquisitely imagined horrors scarcely approached what he endured each day.

Jeffrey Meyers tells this story briskly and sympathetically—perhaps to a fault. While acknowledging his subject's shortcomings, Meyers shows how other people—the stepfather, the publishers, the fickle public, the literary groupies—contributed to Poe's ordeals by denying him the indulgence due a genius. The point is well taken, particularly at this convenient remove, but Meyers's focus on the insensitivity of others renders Poe's penchant for self-destruction all the more unclear. Poe was no doubt looking within when in "The Black Cat" he describes a man who, after torturing and killing his pet, slays his wife as well. His actions, he explains from his jail cell, had been driven by a "spirit of peevishness"—an "unfathomable longing of the soul *to vex itself*—to offer violence to its own nature." So, too, with Poe. But whence this longing? What wound cut so deeply?

And what was the relationship between the social misfit and the society drawn to his tortured musings? It is clear that Poe's work in many ways stroked the romantic sensibilities of his middle-class readers; Poe has long been criticized for his perfervid sentimentality. Yet in many respects Poe's art, and indeed his life, constituted a challenge to the emerging Victorian culture. His graphic portrayal of decomposing corpses, for example, came at a time when liberal ministers were describing death as a pleasant release from the cares of life, and obliging morticians effaced as best they could all that might suggest otherwise. Meyers often skims details from the surface of the past, but he shows little indication of thoughtful immersion in deeper social and intellectual currents.

Within the narrow constraints of his argument, however, Meyers is far more successful. He demonstrates that although misery pervaded Poe's art and made it distinctive, his remarkable creativity persisted nevertheless. Despite his faults, Poe was an able editor and a talented writer, if not most of the time, at least enough of the time to have left a deep imprint on twentieth-century literature. Of this, as Meyers shows, there can be little doubt.

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WILBUR R. JACOBS. *Francis Parkman, Historian as Hero: The Formative Years.* (American Studies Series.) Austin: University of Texas Press. 1991. Pp. xvii, 237. $27.50.

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Of the four great romantic historians in nineteenth-century America, George Bancroft, William Hickling Prescott, John Lothrop Motley, and Francis Parkman, only Parkman continues to have appeal. For a historian, he has received an inordinate amount of attention (five biographies and a large number of specialized monographs and articles), and yet his most recent biographer can still insist that "Francis Parkman deserves to be more thoroughly known than he is" (Robert L. Gale, *Francis Parkman* [1973]). Perhaps the ultimate compliment regarding Parkman's popularity and influence is the controversy he has inspired. He has been criticized for misreading some of his sources, chided for his social conservatism and religious biases, and held to account for his failure to anticipate views judged to be correct by late-twentieth-century dogmatists. Yet most critics and admirers agree that Parkman knew how to write history, that he wrote history to be read, not by a few highly trained specialists but by a broad and intelligent audience. He was, in other words, not only a historian but also a man of letters, deserving of his recent inclusion in the pantheon of American letters, *The Library of America*.

Wilbur R. Jacobs's current entry into Parkmaniana (or "Parkmania") rests on a long and intimate association with the historian and his works. In his own fine studies of Anglo-French rivalry and of Indian-white relationships on the eighteenth-century colonial frontier, he traveled over some of the same ground traversed earlier by Parkman, in the course of which he gained perspective on the latter's interpretive framework and use of sources. Close studies of Parkman's historical and literary methodologies followed, culminating in Jacobs's two-volume edition of Parkman's correspondence. Although Jacobs's concern for Native Americans tended to focus his interest on Parkman's "formative years," the period of *The Oregon Trail* (1849) and of the *History of the Conspiracy of Pontiac* (1851), he has not neglected the later years.

The central theme of his study, Jacobs tells us, is that "Parkman considered himself a hero figure and transposed this novelistic image onto historical figures he studied" and that "he purposely cultivated a heroic image of himself for future biographers" (pp. x, 3). That Jacobs falls short in the development of this theme may be the result of an overreliance on three of his early articles, augmented by "eight additional essays" (p. ix). Thematic coherence has given way to a rather random sampling of Parkman's life and work: his "conquest" of his physical disabilities (what he called the "enemy"), in the discussion of which Jacobs invokes the aid of modern medicine and psychoanalysis; the composition of the *History of the Conspiracy of Pontiac*, despite its flaws and errors of interpretation "one of the most significant books on Indians ever written" (p. 45); his literary devices, which he deliberately employed "to capture reader interest" (p. 83), often at the expense of historical accuracy; and his "sometimes offensive social ideas" (p. 102), racial stereotyping, masculine chauvinism, unsympathetic and even humiliating portrayals of women, and his "social-Darwinist ideals of social progress" (p. 147). Jacobs measures Parkman's work against the standards of modern scholarship and moral values and finds it wanting, an assessment that sounds harsher than he may have intended. There is, after all, little that is new in these criticisms. What Jacobs only suggests but could have emphasized more is Parkman's role as an eloquent spokesman of romantic America, as a historian who viewed the past on a grand scale, who conveyed the drama of clashing imperial rivalries and civilizations in conflict by placing his readers in the midst of the action, whose romantic interest in origins and exotic people and places resulted in early studies of woodland Indian ethnology, and whose skill as a painter with words captured the beauty and grandeur of America's primeval landscapes. Jacobs ends his study with a welcome reprinting of Parkman's 1844 commencement oration, "Romance in America," important not only for revealing "the springs from which his later work flowed" but also for providing "new insight into his romantic concept of history" (p. 170). For all his apparent shortcomings, Jacobs concludes, Parkman stands firm as a "significant part of our literary heritage" (p. 167).

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BRUCE A. MCCONACHIE. *Melodramatic Formations: American Theatre and Society, 1820–1870*. (Studies in Theatre History and Culture.) Iowa City: University of Iowa Press. 1992. Pp. xiv, 320. Cloth $39.95, paper $15.95.

Bruce A. McConachie's study is a valuable attempt to connect various subspecies of American melodrama with particular class groups and their ideologies. Because growing spectatorship allowed steady expansion of aesthetic and viewer differentiation, McConachie is able to explore plays and audiences for what they reveal about each other. Despite the "American" in the title, McConachie's work is avowedly about the Northeast and, in fact, about New York City, with a few asides on Boston and Philadelphia.

McConachie's approach is highly schematic, with a trio of plays chosen rather arbitrarily for each of his groups: mercantile capitalist paternalists who supported "fairy-tale plays" (1820–35); "tradition-directed workers" (in the introduction labeled, very differently, "male Jacksonians of all classes"), their "yeoman ideal" responding to Edwin Forrest's "Napoleonic aura" and to "apocalyptic melodramas" with "deviant avengers" (1830–50, pp. xii, 85, 119, 140); insecure people "straddling the class line" who found in the American Museum's reform dramas "commodified repressed respectability" while they were imbued with bourgeois notions that "unrespectable de-

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viants below the class line . . . fully deserved their misery" (1845–70, pp. 195–96); and "business-class spectators" who found in "sensation melodrama" justification of the "hegemony of the American bourgeoisie" and their own "burden of repression" (1855–70, pp. 230, 257).

The book's problems center in the absoluteness of its divisions between both types of plays and audience groupings. McConachie offers so slight an idea of melodrama's general shape and ideology that he commonly attributes to particular "formations" what was a general trait with general appeal. Those plays focused on female chastity that he associates with paternal mercantilists were staples throughout the century, indeed the moral-plot fulcrum for melodrama of all kinds. The "chance" and "accident" he discovers in bourgeois drama from the 1860s were equally apparent in the earliest of these plays of "Strange Coincidence!" "Deviant avengers" were common in all stages of melodrama because the pallid perfection of most heroes made male stars depend on heroic villains or good people driven to exciting excess or destructiveness by wrongs suffered. Figures enunciating moral truth did not disappear with "paternalist" drama or change to become McConachie's "rationalists" of reform plays. Melodrama always had great variety and changed much, especially toward "realism," first in its greater concern about contemporary problems like alcoholism, poverty, bourgeois greed, and slavery, and then in its softening of hero-villain extremism as long as female chastity upheld what was always a moral allegory. McConachie's structure, however, suffers from not seeing the form's continuities and the cross-class appeal of almost all its formations.

The book is full of unsubstantiated and dubious assertions: that Forrest acted primarily in working-class theaters; that his prize plays were "Napoleonic" (although they denounce all interest in glory and power); that neoplatonism was an important social and dramatic influence; that audiences at the "middling" Bowery theater were working-class men who others saw as "social deviants"; that "apocalyptic dramas" flourished only in working-class theaters; that Catholics were not a part of the Museum's audiences. This list, alas, is vastly expandable.

Yet for all such problems, McConachie's book is a worthwhile study, thoughtful in its general concerns, up to date in its scholarly resources and vigorous in its argument. Especially good is McConachie's recognition of the darker side of melodrama and the insecurity and bleakness within its cheerful assertions. McConachie suggests more sophisticated directions melodrama history should take. That these studies will probably attack as much as they support his conclusions does not lessen the value of the approach he urges and illustrates.

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Tyler Anbinder. *Nativism and Slavery: The Northern Know Nothings and the Politics of the 1850s*. New York: Oxford University Press. 1992. Pp. xv, 330. $39.95.

Tyler Anbinder's impressively researched study provides the first book-length history of the northern Know Nothing Party. Retilling familiar ground, he unearths fresh and valuable information and makes a welcome contribution. He also jumps enthusiastically into a historiographical controversy about northern politics in the 1850s. Readers should know in advance that I take the other side in that debate.

Despite dust jacket claims, Anbinder's study is not comprehensive. He does not study all northern states, and his omission of New Jersey, the dark hole for historians of that decade, is especially disappointing. He also eschews systematic district-by-district analysis of congressional elections. Thus, he fails to identify exactly which northern congressmen elected in 1854 and 1855 were Know Nothings or how many Republican congressional nominations in 1856 were granted to one-time Know Nothings in order to lure nativist support.

This latter oversight reflects Anbinder's interpretive slant. He pointedly rejects recent accounts of Know Nothingism by William Gienapp, Joel Silbey, and myself which maintain that nativism, anti-Catholicism, and prohibitionism, and not simply antislavery sentiment, fueled the voter realignment of the 1850s and later contributed to Republican success. Instead, Anbinder echoes the argument of his mentor Eric Foner that slavery was the preeminent issue of the decade. Anbinder, indeed, goes beyond Foner. Not only did Republicans make no concessions to nativist principles in winning over former Know Nothings between 1855 and 1860 but also antislavery sentiment, not nativism, destroyed the Whig Party, explained why Know Nothing membership mushroomed only after passage of the Kansas-Nebraska Act, and accounted for the votes Know Nothings won in 1854 and 1855 when they outpolled exclusively antislavery parties. The slavery issue therefore caused both the rise and the fall of the Know Nothing Party.

As forthrightly as Anbinder states his case, he often wavers and contradicts himself. Consequently, the argument is neither as compelling nor as original as he implies. Within two pages, for example, Anbinder makes diametrically opposite assertions about whether those who joined Know Nothing lodges in the summer of 1854 were consciously abandoning their old parties and searching for new antislavery vehicles (pp. 41–42). Elsewhere Anbinder amasses overwhelming evidence that Know Nothings were impelled by anti-Catholic, antiparty, prohibitionist, and antislavery sentiments, and he himself often stresses that combination of motives (see, for example, pp. 67, 185). Yet the historians whom he repudiates argue precisely that. None of them denies that most northern Know Nothings opposed slavery's extension. Rather they argue that voters' preference for

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the Know Nothings over single-issue antislavery parties proves that they demanded a political outlet for ethnoreligious antagonisms as well as antislavery sentiments. To counteract his agreement with interpretations he seeks to refute, Anbinder just as frequently ignores his own evidence by insisting that the slavery issue alone explains Know Nothing success (for example, pp. 100, 186, 192).

At other points Anbinder's case rests on unprovable assertions and strained exaggerations of his differences with historians about the relative influence of various factors generating Know Nothing success. (Compare, for example, Anbinder, pp. 55, 67, with Gienapp, *The Origins of the Republican Party* [1986], 100–01, 146–47.) The problem is not that different factors in combination might have different weights. It is that accurate weighting depends on assessments of voters' priorities that are impossible to verify.

Nor are Anbinder's other contentions fully demonstrated. His most original contribution, a comparison of Know Nothing membership in terms of occupation and wealth with "check groups" of non–Know Nothings drawn from census records, aims at refuting Robert Fogel's and my argument that economic dislocation made the Know Nothings more attractive than other parties to poor working-class voters who blamed immigrants for their economic immiseration. But his hard-earned data do not prove his point, for they compare Know Nothings with the total population, not other parties, and say nothing about the economic condition and expectations of Know Nothings during the stagflation of 1854–55. In addition, although he argues persuasively that Silbey and Gienapp have exaggerated the significance of Republican voter registration laws and platform planks in winning over Fillmore voters after 1856, the contention that Republicans made no concessions to Know Nothing principles cannot be proved without systematically surveying Republican newspapers for anti-Catholic appeals, a survey that neither Anbinder nor Foner has attempted.

Anbinder's book, in sum, contains important information and will deservedly become a standard source on northern Know Nothings. Whether it marks a successful "reinterpretation" of northern politics in the 1850s remains debatable.

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HOWARD JONES. *Union in Peril: The Crisis over British Intervention and the Civil War*. Chapel Hill: University of North Carolina Press. 1992. Pp. xiii, 300. $34.95.

Howard Jones has written a solid work on Anglo-American diplomacy that should take its place on the shelf with the timeless studies of Frank Owsley (*King Cotton Diplomacy* [1931]) and Jay Monaghan (*Diplomat in Carpet Slippers* [1945]). Jones offers a fresh revision to those reasons presented by Owsley, Monaghan, and others on why England failed to intervene in the American fratricidal struggle.

Jones focuses on the point that throughout the war British Prime Minister Lord Palmerston steadfastly held his divided Cabinet on a strictly neutral, noninterventionist course. Palmerston and other "British leaders remained opposed to challenging the Union blockade and risking war with a nation that had an army larger than England's and . . . ironclad vessels" (p. 136). He further points out that neither the British nor the Americans, northerners and southerners alike, could ever understand the other's position. Although this remained frustrating for all, it contributed to England's determination not to waiver from its course. Thus, from the beginning of the war, Britain's neutrality played to the benefit of the Union, a fact that the British never fully comprehended nor appreciated.

Like most historians of the Civil War, Jones agrees that the greatest diplomatic crisis of the war came in the autumn of 1862. He does not agree, however, that the southern failure at Antietam and Abraham Lincoln's preliminary Emancipation Proclamation of September 22 tipped the balance against British intervention. He argues strongly that these two events stirred British sentiments in favor of the South. He presents evidence that the pronouncement against slavery was viewed in England as a hypocritical move to incite servile insurrection in a desperate attempt to win the war.

The question of European intervention had come before Parliament on a motion by William Lindsay in June and was the topic of a Cabinet debate on November 11. Four days before the Cabinet meeting George Cornewall Lewis, the quiet, scholarly, and highly respected secretary for war, circulated a thoughtful memorandum among the Cabinet members in which he outlined the dangers associated with intervention and brilliantly argued that the South had not earned a right to recognition under international law. His persuasive points blunted the efforts of Chancellor of Exchequer William Gladstone for intervention on behalf of the South, and of Foreign Secretary Earl Russell's call for a six-month armistice in the American conflict.

Jones concludes that Lewis's efforts and Palmerston's determination to steer a neutral course carried the day. In explaining the prime minister's position as stemming in part from his concern that Canada was indefensible and the possible outbreak of war in Europe, Jones gives credence to Samuel Flagg Bemis's well-established thesis that "European distresses work for American successes."

There are several areas that perhaps deserve greater attention than Jones has chosen to give to them. He gives little space to the influence the pro-North, unemployed textile workers had on the British decision to remain neutral. There is evidence that they associated the northern cause with that of democracy, a goal they held for themselves. There is no

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mention of the impact that northern grain might have had on helping relieve the suffering of these laid-off workers. Although he credits the Union with confiscating more than two million bales of cotton during the war, he does not mention that much of it was sent to England. Research in these areas may reveal that these developments were factors in the British government's decision not to intervene.

Jones's book combines a delightful writing style with excellent bibliography and footnotes. It is based on solid research, primarily in original sources. It is a work that will serve well both the scholar and the general reader.

JAMES W. DADDYSMAN
Alderson-Broaddus College

WILLIAM L. SHEA and EARL J. HESS. *Pea Ridge: Civil War Campaign in the West.* Chapel Hill: University of North Carolina Press. 1992. Pp. xiii, 417. $29.95.

In their book, William L. Shea and Earl J. Hess have set a new standard of historical excellence with their description of this campaign and battle of the Civil War. It is the first detailed account of this somewhat neglected campaign, and in their effort the authors have closely approached the ideal of a complete study of such a campaign.

The book begins with a description of the march of Samuel Curtis's Army of the Southwest from Rolla, Missouri, into Arkansas, and also of the advance of Earl Van Dorn's Army of the West from the Boston Mountains to its meeting with Curtis's army at Pea Ridge. This is followed by a dramatic account of the terrible fighting at Pea Ridge and a description of the withdrawal of Van Dorn's army to Van Buren, Arkansas. The march of Curtis's army across southern Missouri and northeastern Arkansas to Helena is also described. The book closes with a military analysis of Pea Ridge, and a consideration of the cultural legacy of the Pea Ridge campaign. Following the text are forty-five pages of notes and twenty-two pages of bibliography.

The authors' research is of the highest order and includes the examination of manuscripts found in the archives of twenty-one states and the District of Columbia, newspaper files in forty-two cities, and hundreds of publications. In addition, the authors have personally examined all parts of the battlefield and the surrounding country, and their descriptions of the roads and terrain, together with the excellent maps accompanying the text, enable the reader to understand the Battle of Pea Ridge to a greater extent than is generally possible in accounts of most Civil War campaigns and battles.

In preparing this definitive work, the authors have skillfully combined the results of a meticulous examination of virtually all aspects of the Pea Ridge campaign. These include brief resumés of the military backgrounds of the higher officers, evaluations of leadership, logistics, care of the wounded, personal differences among the officers, and ethnic and regional conflicts among the officers and men.

Unlike in many descriptions of battles, tactics at Pea Ridge are discussed down to the company and battery level, and this brings the reader closer to the personal involvement of the common soldiers. Numerous quotes and information taken from letters and diaries of officers and men are inserted into the text to give a clear picture of how they felt and what they saw during the fighting. All of this, together with the excellent detailed account of the battle, enables the reader to understand, to a remarkable degree, the nature of a Civil War engagement. With some imagination, in reading this book, it is possible to gain a sense of being among the men as they struggled in the woods, fields, and ravines of Pea Ridge during the winter days of January 1862. This book is assured of a place among the best of all studies that have been published on Civil War campaigns.

FRANK J. WELCHER
EMERITUS
Indiana University, Bloomington

STEPHEN W. SEARS. *To the Gates of Richmond: The Peninsula Campaign.* New York: Ticknor and Fields. 1992. Pp. xii, 468. $25.00.

Of all the major military campaigns of the American Civil War, the Peninsula campaign of 1862 remains one of the least known. Yet Stephen W. Sears reminds us that this campaign, lasting 160 days, was arguably the largest in the conflict, with approximately 250,000 troops involved. Major General George B. McClellan's plan boldly called for transporting the Army of the Potomac and its supplies from the environs of Washington, D.C., by water to the tip of the Virginia Peninsula, southeast of the Confederate capital at Richmond. From the vicinity of Fort Monroe, McClellan's army would then advance overland to its objective, possibly arriving before the Confederacy could concentrate sufficient forces to defend Richmond. Although the massive logistical effort required to implement the plan worked flawlessly, McClellan quickly demonstrated that he was incapable of conducting decisive field operations. By the time he was ready to push through the initial Confederate defenses around Yorktown, his opponents had both concentrated their forces and withdrawn to safety. McClellan's pursuit soon degenerated into a glacially slow advance that eventually assumed the characteristics of a siege. As the Federal momentum dissipated, Confederate commanders seized the initiative. General Joseph E. Johnston's first effort, at Seven Pines, resulted in an amateurish, indecisive Confederate success and his loss to wounds. Johnston's successor, General Robert E. Lee, reorganized and strengthened the Confederate army,

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then mounted a series of offensives collectively known as the Seven Days battles. By early July 1862 the Army of the Potomac had been driven from the gates of Richmond to the shelter afforded by the Union navy at Harrison's Landing on the James River. At horrendous cost to both sides (a total of 55,820 casualties), Lee had saved the Confederate capital, tarnished McClellan's image, and begun to build his own reputation as one of the world's great captains.

Although a brief synopsis of events in the Peninsula campaign can be found in numerous general histories and biographies, the campaign has received little recent serious study. Sears begins to rectify that omission with this volume. He has long made the dissection of George McClellan's personality and military career his forte, with a previous work on McClellan's Antietam campaign, a full-dress biography, and an edited version of the general's papers. For those familiar with these earlier efforts, the current work will produce no surprises. The same McClellan is present in them all: brilliant conceptualizer, egotistical personality, self-delusive commander, and weak battlefield leader. The valor of Confederate arms and the ability of Lee notwithstanding, Sears clearly indicates that the Peninsula campaign was McClellan's to lose, and he lost it in shameful fashion. Although the focus of the work is most often at the higher levels of command, Sears does not neglect the stories of those struggling in the ranks, who appear through numerous first-person vignettes, often from unpublished or seldom used sources. All of this is crafted into a seamless narrative of great power. Sears leaves to others the minute analysis of individual battles at the regimental and brigade level; instead he is more interested at the epic nature of the struggle and the accompanying tragedy. Perhaps other scholars will now look anew at this significant campaign; few will aspire to tell the story with the narrative skills Sears displays.

WILLIAM GLENN ROBERTSON
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Fort Leavenworth, Kansas

ERNEST B. FURGURSON. *Chancellorsville 1863: The Souls of the Brave*. New York: Alfred A. Knopf. 1992. Pp. xv, 405. $25.00.

This is a masterful account of the myriad tactical moves and troop encounters that made up the Battle of Chancellorsville, fought the first week of May 1863 in the Wilderness region of northern Virginia. In this battle, Major General Joseph Hooker led a Union army to defeat against the numerically inferior forces of Robert E. Lee.

Ernest B. Furgurson is at his best in describing the detailed action of the battle in a journalistic style that gives the reader a vivid, immediate sense of what it was like to fight at Chancellorsville. This effect is achieved partly through the author's skillful quotation from a wide variety of eyewitness accounts, written by participants ranging from the lowest to the highest ranks. No previous volume can match the clarity of Furgurson's retelling of the course of events at Chancellorsville. This is the author's contribution. Eighteen excellent maps aid in the execution of the task.

But here the book's achievement ends. Like too many Civil War books, this is ultimately only a polished retelling of a twice-told tale. The book fails to place events in a larger context. The author's claim that the South's victory at Chancellorsville actually led to ultimate defeat in the conflict is too sweeping and shallow a generalization. It is hackneyed and debatable to argue, as Furgurson does, that T. J. "Stonewall" Jackson's death at Chancellorsville, and Lee's overconfidence after that victory, lost the rebels Gettysburg and therefore also the war.

Furgurson is uncomfortable when he must venture beyond sparkling narrative. Thus, he handles poorly the question of why a skillful strategist like Hooker performed so badly in this battle, falling back on the old argument of a weak personality inclined to heavy drinking. Important works published in the last fifteen years that throw light on the larger questions are not in the bibliography. One example is Herman Hattaway and Archer Jones's study, *How the North Won* (1983). It is peculiar that Civil War writing does not seem to be held to the same scholarly standards as other areas of the discipline, where knowledge of recent works is expected and where detailed reference to sources is essential. It is almost as though the war functions as folklore, the epic tradition of male culture, much as Beowulf and Roland functioned for the warriors clustered in the mead hall. What matters is a rattling good recounting of the grand old story rather than a serious contribution to historical understanding.

Much Civil War writing is out of step in another way. It remains insensitive to issues of race. About the only times that African Americans are mentioned in this book, they appear in the condescending and often derogatory observations of white people who were amused by the wretchedness of displaced slaves. This is not a matter of political correctness but of professionalism: we fail our scholarly standards when we make light of the trauma that the Civil War brought to black as well as white Americans.

Attractively written and produced, this book will appeal to buffs but adds little to our greater understanding of the Civil War or of modern military history.

MICHAEL C. C. ADAMS
Northern Kentucky University

PETER COZZENS. *This Terrible Sound: The Battle of Chickamauga*. Assisted by KEITH ROCCO. Urbana: University of Illinois Press. 1992. Pp. xii, 675. $39.95.

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Not every battle, not even the big ones, can be seen as decisive or as turning points in wars. Such a description fits the two-day battle of Chickamauga, fought in the Civil War along the Chickamauga Creek in northwest Georgia on September 19 and 20, 1863. The ultimate prize of extended combat in the region was control of the railroad hub of Chattanooga, Tennessee. The battle is covered in some fashion in one-volume and multivolume histories of the Civil War and in what has been, until now, the standard account, Glenn Tucker's *Chickamauga: Bloody Battle in the West* (1961). The Confederacy's morale went up some following the engagement, but Chickamauga was a hollow victory for General Braxton Bragg's Confederate Army of Tennessee. It gained neither key terrain nor crippled the opposing Union Army of the Cumberland, commanded by General William S. Rosecrans. Although the Confederates won the battle, their losses (16,000 killed, wounded, and missing) were serious and could not be readily replaced. The Union Army, also suffering serious losses, was reinforced and given a new commander, General George H. Thomas, who had become known by the sobriquet, "Rock of Chickamauga," for the stand part of his corps made during the battle.

Most of these conclusions about the battle will be familiar to scholars and general readers alike, but the value of Peter Cozzens's work lies in the pleasing writing style he uses to retell the story, the clarity of his analysis, and the depth of his research, which surpasses that of Tucker. Cozzens handles unusually well the intricate mosaic of a big battle, clearly recounting the movement of individual units and relating them to the broad picture of the conflict as it unfolds. Cozzens introduces officers on both sides in polished vignettes, some of which open chapters and personalize the account. He delivers astute evaluations of the principal leaders on both sides. For example, Cozzens adds to what most historians of the battle have argued, saying that Bragg was a vacillating, deficient field general, describing him with such adjectives as "dazed" (p. 23), "disingenuous" (p. 170), and "lackadaisical" (p. 301). He also emphasizes that Bragg's mercurial health undermined any ability he had as a combat commander. Therefore, Cozzens is in accord with the study by Judith Lee Hallock, *Braxton Bragg and Confederate Defeat*, volume 2 (1991), but in contrast with Steven Woodworth's *Jefferson Davis and His Generals* (1990). General Leonidas Polk also suffers at Cozzens's hand, coming across as a "master of intrigue" (p. 534) whose lack of aggressiveness and inability as a corps commander cost the Confederacy dearly. Cozzens reveals the weaknesses of senior officers on the Union side. For instance, the pressures on Rosecrans, who was "impulsive and excitable" (p. 9), created a condition in which the Federal commander "was coming unhinged" (p. 168) by the burdens of his responsibilities. Thomas, however, was a "methodical and phlegmatic" (p. 9) general who picked his ground carefully and fought well. Cozzens contributes sound assessments for other officers, including generals James Longstreet (who comes off not as favorably as some would like) and James Garfield (who played politics while acting as Rosecrans's chief of staff).

In the course of this long book, Cozzens confirms some of the conclusions of Thomas L. Connelly. In particular, Cozzens concludes that the high command of the Army of Tennessee was so rocked by dissension that it was remarkable that it functioned at all. The book contains evocative line drawings by artist Keith Rocco, several good maps updating the development of the battle, and an excellent index. This is the new standard study of Chickamauga, and one that may be hard to surpass.

JOSEPH G. DAWSON III
Texas A&M University

ALBERT CASTEL. *Decision in the West: The Atlanta Campaign of 1864*. Assisted by LAURA KRIEGSTROM PORACSKY. (Modern War Studies.) Lawrence: University Press of Kansas. 1992. Pp. xvi, 665. $29.95.

This study of the Atlanta campaign by Albert Castel, a veteran Civil War historian, falls into the "face of battle" and "fog of war" genre. Castel's detailed narrative focuses on command characteristics when face-to-face with the enemy in a protracted campaign of confrontation and movement. His major contribution is his attempt to weigh the role of military personality and basic military judgment in this elaborate reconstruction of the decision-making process. In what is obviously a labor of love, the author takes his field commanders and their soldiers with painstaking care through every mile of forest, every inch of mud, every day of rain or sweltering heat, and every maneuver and battle from the first day of the campaign to the last.

On the one hand, Castel develops a picture of field commanders who, with the exception of John Bell Hood and George Thomas, were wary and cautious. On the other hand, he portrays commanders who by 1864 had learned that battles to a degree fight themselves in the "fog of war." Although he allows for the inherent limitations of command and control, Castel is to a greater or lesser degree critical of the balance of temperament and basic military judgment of Joseph Johnston, James McPherson, Oliver Otis Howard, and especially William Tecumseh Sherman, to note merely those who held some degree of independent command. He concludes that Sherman's repeated failure to exploit opportunities to mangle if not defeat Hood's army is attributable to a military personality disposed to raiding rather than to fighting. Castel gives high marks to George Thomas as a battlefield general, and he is more sympathetic to Hood than most. In specific actions Castel sees Hood more as a victim of luck in the chaos of battle than a victim of poor military judgment. Castel sees Hood's

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dilemma as the strategic necessity of trying to do what his army could not. Castel's conclusions certainly will stir the pot of controversy.

Castel's well-written and absorbing study may well stand as the definitive battle narrative of the Atlanta campaign. One cannot in a brief review do credit to how Castel's narrative consciously links with other contemporary studies that are building a modern explanation of that campaign and its place in the Civil War. To fill out other facets of the "fog of war" and the "mask" of mid-nineteenth-century field command relevant to the Atlanta campaign, one will benefit from reading this study in conjunction with recent works by Richard McMurray on Confederate strategy and leadership in the West, Jeffrey Lasch on Joseph Johnston's use of railroads, Charles Royster and Charles Vetter on Sherman, and my own study of field transportation and some other organizational problems of field command.

EDWARD HAGERMAN
York University
Toronto

JOSEPH P. REIDY. *From Slavery to Agrarian Capitalism in the Cotton Plantation South: Central Georgia, 1800–1880.* (The Fred W. Morrison Series in Southern Studies.) Chapel Hill: University of North Carolina Press. 1992. Pp. xiv, 360. $45.00.

This is one of an increasing number of studies about the transition from slavery to freedom in the nineteenth-century South. Although Joseph P. Reidy's subject is central Georgia, his material is altogether familiar: the rise of an antebellum planter class, the developing paternalism of the master-slave relationship, the tensions between slaveholders and yeomen, the crisis in the 1850s, the participation of slaves in the process of emancipation, the immediate postwar struggles over the contract labor system, the emergence of black churches and schools, the shift to Radical Reconstruction, the origins of the black franchise and black political participation, electoral fraud and violence, and, finally, the emergence of an agrarian bourgeoisie and a rural proletariat.

Equally familiar, if not downright tiresome, is the thesis into which Reidy forces his material. He begins with the "precapitalist labor relations" of an "organic" slave society in which "reciprocal obligations" marked the ties that bound master and slave as well as slaveholder and yeoman. The Civil War destroyed this society, opening the way for the protracted series of conflicts that issues in the "agrarian capitalism" of the late nineteenth century. Although Reidy places his study in the context of other modern slave systems, nothing seems to have happened in central Georgia that alters what we already knew.

Reidy explicitly defines capitalist social relations as the separation of the worker from the means of production, but since this would automatically render the slaves a capitalist proletariat, he implicitly shifts to another definition: capitalism equals wage labor. From that premise the iron laws of logical necessity take hold, and Reidy's thesis virtually produces itself: slavery was not a system of wage labor, therefore slave society was not capitalist, therefore the transition from slavery to wage labor constituted a "capitalist transformation." One need not dispute the tremendous significance of wage labor to wonder whether this logical construction purchases simplicity at too great a cost. Rather than see capitalist development as a centuries-long process marked by countless struggles (and several revolutions), with no clear beginning and no single historical trajectory, Reidy posits a capitalist "big bang" in which all that came before the rural proletariat and the agrarian bourgeoisie was fundamentally antithetical to capitalism. So much for absolute property, bourgeois patterns of social reproduction, the liberal state, and the fetishism of the commodity, all of which were indissolubly linked to the development of modern slavery and deeply intertwined with the way slaves and masters produced their cotton and reproduced their way of life in central Georgia. Yet Reidy imagines the relationship of slavery and capitalism in the stark terms of irremediable hostility (he crudely divides historians into those who see the Old South as capitalist and those who do not), while his own evidence suggests a more complex, contradictory relationship marked at once by close interdependencies as well as sharp tensions.

Reidy's otherwise worthwhile evocation of the struggles of the postbellum South is thus undermined by his relentless determination to shove his antebellum evidence into simplistic categories that simply do not work. For example, as evidence of the slaveholders' alleged contempt for the "capitalist system," Reidy cites planter attacks on the "acquisitiveness" and "money-grubbing" of northern society—as though such complaints were not a staple of bourgeois reform, as though the narratives of ex-slaves did not make precisely the same argument about the masters, and as though greed were the essence of the "capitalist system." Reidy is no better on paternalism, whose emergence and decay is one of his recurring themes. He argues that the rise of antebellum paternalism was marked by the replacement of material rewards for physical coercion as a means of securing slave obedience. But he provides not a shred of evidence for a decline of brutality or a rise in "material rewards," for no such evidence exists. On the contrary, there is good reason to believe that in the generation before 1860 the slaves suffered from increasing levels of malnutrition, lower birth weights, more stunted growth patterns, higher infant mortality, and lower life expectancy. Things changed after the war, but Reidy is not interested in that part of the transition.

JAMES OAKES
Northwestern University

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MARGARET HUMPHREYS. *Yellow Fever and the South.* (Health and Medicine in American Society Series.) New Brunswick, N.J.: Rutgers University Press. 1992. Pp. x, 226. $45.00.

With a handful of notable exceptions, historians of public health in the United States during the nineteenth century have generally taken one of two lines of inquiry. The first, with less than convincing results, has tried to highlight or ennoble what nevertheless remained a skimpy record; the second has recognized the record for what it was and asked implicitly why so little was done. But the assumptions underlying both lines of inquiry have been fundamentally similar, and the consequence has been a body of historical work remarkably plaintive in tone.

Margaret Humphreys takes a carefully measured but significant step in a useful new direction. By delimiting her study both regionally and chronologically (approximately 1840 to 1905), she is able to look in detail at what actually was done and why. What she finds is a direct relationship between a single disease, yellow fever, and sputtering efforts at public health in the southern states; a relationship that existed not because the disease was the region's greatest human scourge (although terrifying in epidemic waves, yellow fever was never the South's biggest killer), but because the disease, commonly thought to be spread by cargo as well as by contact, was the region's greatest commercial scourge. Without yellow fever's peculiar traits, the southern states would have developed essentially no public health infrastructure at all. And most of what was built during the late nineteenth century, Humphreys points out, was promptly dismantled after 1905, as soon as yellow fever was under control.

In working her way through the bureaucratic infighting, intraregional rivalries, muddled scientific debates, medical uncertainties, professional power plays, business pressures, and personal jealousies that characterized this relationship in the South, Humphreys throws new light on such shadowy subjects as quarantine politics, the role of the federal government in public health, and the economic implications of epidemic disease. Walter Reed's demonstration of the mosquito vector for yellow fever and the role of the army in Cuba, subjects often portrayed in heroic isolation, are here placed in a nuanced and ironical context. Readers also learn to appreciate the key fact that public health in the late-nineteenth-century South did not always take the form of official activity by duly constituted public authorities: embattled businessmen and panicky citizens often made public health policy of their own. Indeed, Humphreys suggests that authorities often reacted less to yellow fever itself than they did to the commercially disastrous efforts of disparate and frequently feuding southern blocs to combat the disease in different ways that served their separate interests.

In the end, Humphrey's study remains a closely focused monograph, and it is not without its problems. Whose interest is being served by government at any given time, for example, remains repeatedly unclear; and stress on professional prestige as the chief reason for the independent stand of southern state boards of health is less well supported than it might be (some budget figures would have helped). Humphreys shies away from exploring many of the biggest implications of her analyses. But as a monograph, this is an excellent and heuristic piece of work.

JAMES C. MOHR
University of Oregon

ROBERT A. MARGO. *Race and Schooling in the South, 1880–1950: An Economic History.* (Long-term Factors in Economic Development.) Chicago: University of Chicago Press. 1990. Pp. x, 164. $24.95.

The value of Robert A. Margo's study of schooling and race in the South between 1880 and 1950 is found in two dimensions. The first is the valuable data set on southern illiteracy, school attendance, years of schooling, and per-pupil expenditures by race. The second is the extended econometric analysis of this data that constitutes the core of the book. Margo examines the determinants of differential levels of funding and teacher salaries by race. He then reverses causality and asks about the impact of differential funding on disparities between blacks and whites in school attendance, literacy, and, for one state, standardized tests. Finally, he takes up the consequences of these differences on employment and migration.

Margo's findings are fascinating. Pure racial discrimination looms as a major determinant of differences in teacher salaries and the industrial and occupational structure of the black labor force compared to the white labor force. Inferior education, itself in large part the product of racism, negatively influenced black migration from the South. Margo writes that "by limiting the flow of black labor from the rural South, educational discrimination helped perpetuate the traditional existence of a supply of low-wage labor well into the twentieth century, a fact that the southern white elite was keenly aware of" (p. 127). The limited support accorded black education affected school attendance, literacy, and standardized test scores. Margo carefully notes, however, that even equality in education support would not have been enough to achieve parity. A gap would have persisted, a phenomenon that at one point he attributes to "family background variables" (p. 79) but at another says is traceable "ultimately to slavery" (p. 4). Black disenfranchisement in the 1890s is singled out as the reason for the relative erosion that occurred in black education between 1890 and 1910.

Margo believes his results indicate that neither "the human capital" nor "institutionalist" framework is adequate to explain the lag in the relative economic

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status of blacks. By the first he means those who argue that insufficient education caused the problem; by the second he refers to those, including myself, who argue that entrapment in low income occupations was the cause of black impoverishment. Perhaps not surprisingly, I believe that the weight of Margo's evidence supports the institutionalist view. Of course it is true that with more education blacks would have progressed more rapidly. But it is precisely the strength of the "institutionalist" position to identify why such a higher level of education was not present. Advanced levels of black education comparable to that of whites would have been dysfunctional to the southern economy. Cotton did not require high levels of formal schooling and more advanced levels of educational attainment would have widened the array of employment opportunities available to the black labor force. Denying education to southern blacks was thus an important mechanism of entrapment by which the southern plantation economy was rendered viable. Without that deprivation, the plantation cotton industry would have found it more difficult to mobilize the large-scale, low-cost labor force it required.

JAY R. MANDLE
Colgate University

MICHAEL E. ENGH. *Frontier Faiths: Church, Temple, and Synagogue in Los Angeles, 1846–1888*. Albuquerque: University of New Mexico Press. 1992. Pp. xix, 267. $32.60.

The American West is one major geographical area that has traditionally, almost systematically, been overlooked by religious historians. One reason perhaps lies in the absence of the kinds of events and personalities that usually attract historians. Until recently, at least, it has given rise to few new denominations, theological systems, or leaders of national stature within the established traditions. In the twentieth century, its most novel features have been at the fringes, easily dismissable as "cults" of evanescent significance; in the nineteenth century, even these movements (save for the Mormons in Utah and environs) have been largely absent. The distinctive religious experience of the West—or, perhaps, Wests—is not easily visible through the usual heuristic lenses.

In his book, Michael E. Engh takes some major steps toward rectifying this historical imbalance. In his study of the early years of Los Angeles—after it had passed into settlers' hands but prior to its demographic reconfiguration brought about through the transcontinental railroad's impact—Engh characterizes the city as more akin in its social and religious dynamics to other southwestern regions, rather than the Evangelical Protestant–dominated earlier waves of frontiers that had progressed with migration from eastern regions. Evangelical missionaries, in fact, found Los Angeles rather hard ground to break, given their ethnocentric visions and strategies to which the local mixture of Mexican Catholics, Chinese, and even a surprising number of Jews were unreceptive. Although Methodists, Baptists, and others became more numerous and influential as the city's character was changed by subsequent migration, the initial religious mosaic was significantly different from that of most other western cities.

Engh discusses specific aspects of this transitional period, including the role of women—particularly Catholic sisters—in the city's religious and civic life. His treatments of different groups are clearly influenced by the evidence available; while the activities of the Daughters of Charity are documented in almost excessive detail, the religious lives of the Chinese come into focus mainly through the context of intergroup conflict. Interreligious cooperation is another important theme, a phenomenon that yielded to the usual hardening of boundaries as the Catholic community became Romanized under European episcopal authority, and as the Evangelical presence became larger and more aggressive.

Engh's formulation of his themes and his interpretations of them are generally appropriate, although they might have been enhanced by more of some judicious invocation of sociological and ethnological constructs. His focus is also on the social histories of the various Los Angeles subcommunities, with the exception of Engh's perceptive discussion of the enforced transition from folk to "official" Catholicism, in which he deals with religion per se. On the whole, this is a thoughtful and readable contribution to what one hopes will become a larger and more systematic attempt to explore a neglected aspect of America's religious history, one lacking not so much in data as in appropriate interpretive paradigms. Kudos to Engh for a good beginning.

PETER W. WILLIAMS
Miami University
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MARY HURLBUT CORDIER. *Schoolwomen of the Prairies and Plains: Personal Narratives from Iowa, Kansas, and Nebraska, 1860s-1920s*. Albuquerque: University of New Mexico Press. 1992. Pp. xi, 365. $32.50.

For some time now frontier historians have debated whether they should merely narrate a well-written story or, in addition, offer theoretical interpretations. Mary Hurlbut Cordier attempts to do the latter. To assist in the analysis of ninety-six schoolwomen from the three states, however, Cordier relies on the more superficial elements of secondary works, rendering the book less valuable than it could have been.

Essentially, Cordier reiterates the theses of several western historians that refute popular images of frontier women as being one-dimensional. The

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schoolwomen she examines are instead people who led "multidimensional lives" (p. 3). Yet to set the historical stage for these purportedly progressive educators and community builders, Cordier ignores important interpretive works on frontier women. Such studies might have guided her toward a more introspective analysis of the complex web of emotions that undoubtedly were related to how the women "managed their individual pursuits of education [involving selflessness], power, and selfhood" (p. 145). For example, Cordier suggests that greater opportunities in higher education and women's suffrage were inspired by progressives. Yet Julie Roy Jeffrey maintained that these were largely ideological conservative ventures; coeducation had the effect of segregating female students and reinforcing their restricted social roles, and the relatively early suffrage campaigns symbolized male efforts to maintain political control of states such as Idaho by enfranchising their female supporters (*Frontier Women: The Trans-Mississippi West, 1840–1880* [1979]).

Cordier looks at the environmental context in which the subjects' personal and professional experiences occurred, discussing the physical landscape, weather, and periodic problems with pestilence. Yet the purpose of this backdrop is unclear. Important works such as Brian W. Blouet and Frederick C. Luebke, eds., *The Great Plains: Environment and Culture* (1979), and Vera Norwood's "Women's Place: Continuity and Change in Response to Western Landscapes" (in Lillian Schlissel *et al.*, eds., *Western Women: Their Land, Their Lives* [1988]) could have informed Cordier's study, leading to more meaningful interpretations of the relationship between the physical landscape and the schoolwomen's ideology and culture.

In addition, other historians of frontier female educators have investigated the expectation that these women be simultaneously selfless and ambitious. For example, Kathryn Kish Sklar explained that the result could be "invalidism," characterized by periodic physical and emotional breakdowns (*Catharine Beecher: A Study in American Domesticity* [1973]). One of Cordier's subjects, Sarah Gillespie Huftalen, seemed to have experienced such symptoms, but Cordier provides little analysis of Huftalen's account, in which she claimed she was rendered "bloodless from . . . the years of . . . ceaseless care and toil" nursing her aging mother (p. 216).

The intellectually and physically tedious work of collecting and organizing sources and writing a historical narrative should not go unrewarded. When telling a story, however, whether by intention or default, historians rewrite the past in their own image. Thus, Cordier's book would have been enhanced by a greater effort to include the rich body of literature on frontier women.

COURTNEY VAUGHN
University of Oklahoma

ROB KROES. *The Persistence of Ethnicity: Dutch Calvinist Pioneers in Amsterdam, Montana.* (Statue of Liberty–Ellis Island Centennial Series.) Urbana: University of Illinois Press. 1992. Pp. 161. $29.95.

The Dutch were hardly the most numerous immigrants to the United States, and Amsterdam, Montana, lies far from the center of the Dutch-American network. At the hands of Rob Kroes, a historian from the original Amsterdam, however, the town's story comes close to the heart of the American experience.

Histories of Dutch America have tended to be systematic and comprehensive, so this volume's brief compass allows for little substantive revision or theoretical tunneling. Yet it harvests established scholarship in combination with new research so deftly as to make this a most accessible introduction to the group's experience. The intellectual and social structures, the demographics and dynamics of Dutch America are all here, and they apply to its urban and suburban locales as much as to the rural outpost the book studies.

Amsterdam, Montana, was founded a hundred years ago by farmers from the northern Netherlands who were being squeezed out of a strained rural economy. Lowlanders though they were, they soon mastered irrigation farming at a 4,000–foot elevation in the Gallatin Valley. Being as proficient at religion as with water, the Dutch were led—sometimes misled—by Calvinist ministers-turned-immigration agents into linking up with the Reformed ecclesiastical network that defines the enduring Dutch-American subculture. Casting all their cares on the Lord, they also cast all their tensions into theological disputes, managing to produce a schism in the depths of the Great Depression of the 1930s. Far from weakening their community, Kroes argues, the quarrel worked to solidify it, "setting them apart from a social environment that had no inkling of what they were so worked up about" (p. 120).

It is the manner of telling as much as the matter told that sets this book apart as a model of history writing. Kroes assembles his account from census and land-sale records, maps, grave markings, caches of letters, church periodicals, and a hundred personal interviews. After all, cobbling together a life from pieces old and new is what immigration and acculturation are all about. Better yet, the book lets all its seams and bare spots show, like all the lost names of failed settlements that it tries to trace. Happily, Kroes manages this without a bit of postmodernist rhetoric. Rather than discoursing on "text," he etches brilliant images. The immigrants' movements across a vast North American space are suddenly linked by stray bits of information to reveal a network of ethnic enclaves; thus, Kroes manages to chart this immigration's "stations of the cross, a human calvary of trial, adversity, and suffering" (p. 38).

Writing such as this makes Kroes a worthy heir of the Netherlands' American studies tradition of Johan

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Huizenga and Wim Schulte Nordholt, Dutch pilgrims who, like this volume's subjects, have gotten to the heart of the American country.

JAMES D. BRATT
Calvin College

RONALD R. KLINE. *Steinmetz: Engineer and Socialist.* (Johns Hopkins Studies in the History of Technology, number 13.) Baltimore: Johns Hopkins University Press. 1992. Pp. xii, 401.

Even Charles Proteus Steinmetz, Ronald R. Kline tells us, "may not always have been able to separate the myth from the man" (p. xi). Steinmetz, least likely and most obvious of cultural heroes, founded his reputation on theoretical achievements so abstruse that nonprofessionals made little sense of them. Thomas Edison, by comparison, invented everyday marvels, like electric lights and phonographs, whose revolutionary value was obvious. How did this engineer's engineer come to rival Edison's mythic importance in the early 1920s?

Kline's explanation begins with Steinmetz's theoretical importance; he introduced methods sophisticated enough to address the deep questions confronting electrical system designers at the turn of the century without requiring the impractical laboratory conditions of scientific physics. Soon after arriving in the United States in 1889, Steinmetz published the first of hundreds of technical papers and in 1892 began a lifetime tenure at General Electric (GE).

Once discovered by *Success* magazine in 1903, the "little hunchback," who came to America a penniless immigrant and rose to privileged GE status all the while preaching socialism, provided delicious copy for reporters trying to make sense of big business and nearly unintelligible technological systems. GE and socialists alike used Steinmetz for ideological purposes. He cooperated with both, walking a cautiously crooked line as corporate employee and politician theorist. Active in the socialist local in Schenectady, as president of the local school board, and as candidate for state engineer of New York, Steinmetz advocated a corporatist socialism in which the new order would reflect the harmonies and efficiencies of GE: "I look forward to the time when the corporation will be mankind, those times when all mankind will form a co-operative industrial organization which in its initial crude form is represented by the modern corporation" (p. 219). GE's increasingly sophisticated public relations department saw Steinmetz as a symbolic counter to the firm's bureaucratic anonymity; eccentric, gregarious, and brilliant, he provided GE with a human face.

How much did Steinmetz believe his mythic identity? Given extraordinary freedom within GE because of his technical and symbolic value to the firm, he seemed to assume that all employees fared as well. Kline emphasizes this ambiguity as the key unresolved dimension of his character and social role. As hero-engineer, Steinmetz embodies a central tension of the Progressive Era, the search for a workable social order combining egalitarianism with the bureaucratized world of big business. Steinmetz, hoping to solve political problems with an engineer's solutions, was hardly unique. For many who worried how to digest millions of mostly peasant immigrants into an explosively urbanizing and industrializing nation, the gleaming precision of high-tech systems looked better than the older politics of debate, dissent, and compromise. By telling Steinmetz's story in such thorough detail, Kline makes a substantial contribution to understanding this persistent technocratic longing.

Two limitations should be noted. Stylistically, Kline juxtaposes technical, biographical, and political material sometimes without integrating one with another. The reader must work too hard to find all the parts of his best insights, such as Steinmetz's move from sure-footed engineering to naively utopian corporatism. I also wondered how he sees Steinmetz's physical deformity vis-à-vis his politics. Was bonhomie as a substitute for intimate personal love a model for his conviction that mechanized efficiencies work better than fleshy politics? I would have liked to hear about the man himself. Further rumination about this icon of the Progressive Era would have enriched a thoughtful book, helping readers to see Steinmetz's tensions as our own.

JOHN M. STAUDENMAIER, S.J.
University of Detroit Mercy

BURTON W. PERETTI. *The Creation of Jazz: Music, Race, and Culture in Urban America.* (Music in American Life; Blacks in the New World.) Urbana: University of Illinois Press. 1992. Pp. xii, 277. $29.95.

The social history of jazz has been well-served by Burton W. Peretti's excellent and clearly written study. Using a chronological framework stretching from 1900 through 1940, Peretti makes judicious use of oral histories and secondary works to develop a primarily topical approach that synthesizes "three major streams of jazz studies that have proven of limited use to cultural historians: musicology, social science field work, and aficionado history" (p. 3). In general, Peretti succeeds in this effort by locating the creation of jazz within the shifting boundaries of geographical and professional communities, which were often defined in response to larger social divisions based on race.

Beginning with several chapters addressing first the "African and Rural Roots" of jazz, then its evolution in New Orleans and, later, northern cities, Peretti's focus on defining communities within jazz enables him to reframe old assumptions and raise new questions about neglected topics in jazz social history, including, for example, the role of gender and ho-

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mosexuality in the establishment of a male musical fraternity. Peretti gives specific attention to the function of the "color line" on traveling bands in the South, and on northern music communities where white and black musicians mixed more freely. Peretti argues persuasively that gains made toward racial equality within jazz depended primarily on the changing consciousness of individual musicians. He concludes that the legacy of entertainment media such as minstrelsy "presented the advocates of equality in jazz with too many daunting barriers" (p. 210).

Peretti takes issue with histories that neglect to seriously consider the contributions of white musicians to jazz and the influence of Euro-American music on jazz generally. Peretti also challenges the deviant subculture models identified in much social scientific writing on jazz, concluding instead that "extramusical" aspects of musicians' lives, such as drug and alcohol use, dress, and language, reflected racial and cultural tensions generally, and not a "distinct and influential subculture" (p. 144). Instead, Peretti offers an innovative description of how the musical culture of jazz might be best understood by looking at the performance practices shared among musicians, including the definitions of musical literacy that evolved with the music, or the decisions involved in participating in the increasingly commercialized market for jazz.

Each of Peretti's chapters could stand on its own as an essay detailing the ways in which jazz musicians, often responding to the changing demands of their professions, sought to expand the opportunities available for playing jazz. Peretti acknowledges the exploitive conditions musicians faced in employment and the limited benefits accruing from radio and recording contracts. As jazz musicians embraced the urban and commercial context for their music, they, according to Peretti, "confronted both the worst and the best situations the decade could have offered them—an unprecedented economic depression, following by the greatest period of acclaim and success jazz would ever receive" (p. 164).

Peretti hopes that his study of jazz "illuminates the general American culture of the inter-war eras" (p. 2). Although he throws some light on this subject, his book's considerable strengths lie in another direction. Peretti offers richly detailed portraits of evolving jazz communities, where the rural roots of jazz were transformed by urban life.

KATHY J. OGREN
University of Redlands

STEVEN BIEL. *Independent Intellectuals in the United States, 1910–1945*. (The American Social Experience Series, number 25.) New York: New York University Press. 1992. Pp. xvi, 294. $40.00.

By "independent intellectuals in the United States" Steven Biel means specifically that much-studied generation of writers and activists who clustered around Greenwich Village and came of age in the first two decades of the twentieth century. One of his major aims in surveying the lives and writings of Randolph Bourne, Van Wyck Brooks, Lewis Mumford, H. L. Mencken, Margaret Sanger, Emma Goldman, Max Eastman, John Reed, Mabel Dodge, and many others is "to call into question the nostalgic vision of a Golden Age of intellectuals" (p. 3). Biel has read widely and carefully in the voluminous primary sources; he quotes and paraphrases extensively; his contentions—concisely and clearly put—point to a thorough familiarity with the major biographies and standard scholarly works. Following Christopher Lasch's notion of the writer or critic as a "social type," Biel aims at "a cultural history of intellectuals rather than a formal history of ideas" (p. 5).

He opens with chapters on "The Repudiation of the University"—Bourne said a professor's chair was "too much of an invitation to dawdle"—and the intellectuals' preference for a life of the mind that would, they hoped, resolve any conflict between disinterestedness and public influence. Biel then details "The Geography of Intellectual Life" (on New York City and the recurring desire to escape to the countryside or expatriate to Europe); the vital role of women as members of "the critical community;" the desire, passionate in John Dewey and his disciple Bourne, to refashion education; the search with Brooks for the "Uses of the Past" and, in the 1930s, for the correct understanding of Karl Marx. Biel stresses both the tensions inherent in intellectual life and the creativity and connectedness of ideas and personalities; see, for example, his chapter on "An Alternative Morality" on the way sex, feminism, gender, and ingrained assumptions intertwined, but not always smoothly. Their personal lives aside, Biel admires the effort of New York's finest intellectuals to break free from inner and outer restraints and to become public intellectuals with a liberating message. Not surprisingly, he rejects any notion of a golden age or any simplistic view of the "lyrical left."

Of course, no one familiar with the era or with the lives of any of Biel's cast of characters will find his thesis or various subsidiary arguments surprising. Henry F. May elaborated on the tensions, ambiguities, and intellectual complexities of the generation of 1914 in *The End of American Innocence* (1959). So, too, do less encyclopedic works like Leslie Fishbein's *Rebels in Bohemia* (1982), Casey Blake's *Beloved Community* (1990), and John Patrick Diggins's splendid polemic, *The Rise and Fall of the American Left* (1992), an expanded version of earlier writings. Every major "independent intellectual" Biel discusses has been the subject of a thorough, interpretive biography. Still, Biel's book has the merit of bringing to life, in a brisk and accessible format, a brilliant group of men

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and women who preferred to do good rather than well and left a rich legacy of creative thought.

BRUCE CLAYTON
Allegheny College

MORRIS DICKSTEIN. *Double Agent: The Critic and Society.* New York: Oxford University Press. 1992. Pp. xvi, 220. $23.00.

The double agents in Morris Dickstein's title are literary critics who take their stand on what Lionel Trilling termed "the dark and bloody ground where literature and politics meet." Here, Dickstein says, they attempt to combine "a deep feeling for art with a powerful sense of its changing place in human society" (p. xiv), acting as agents both of "a strong sense of literature in itself" and "an equally strong sense of the place of literature in the course of history" (p. xi). They also serve as double agents on behalf of a vital public sphere, speaking in a voice that at once sets itself against both the specialized languages of insular professionalism and the trivial chat of the publicity machines of the culture industries.

Such critics—Matthew Arnold, Edmund Wilson, Malcolm Cowley, George Orwell, F. W. Dupee, Alfred Kazin, and (above all) Trilling—are Dickstein's heroes, and his book of related, and occasionally repetitive, essays is a tribute (if not a panegyric) to them. In the interwar heyday of the American public intellectual, Trilling and other New York intellectuals set themselves equally apart from the formalism of the New Criticism and the social reductionism of Stalinist hacks. Dickstein, in calling on contemporary critics to follow in their footsteps, finds himself doing similar battle with, on the one hand, structuralists and deconstructionist critics who have made a fetish of the "text" and, on the other hand, "new historicists" who have made a fetish of ideological context (or, to be more accurate, the "text" of context). Whereas his predecessors found themselves confronting the "intentional fallacy" and a vulgar reduction of art to the determinations of the means of production, Dickstein must wrangle with the "death of the author" and crude political formulas grounded in identity politics and a numbing mantra of "race, gender, class."

He does so in exemplary fashion, not only pointing up the virtues of those double agents whose sympathies are closest to his own but also lending an attentive ear to the insights of other critics with whom he is more ill at ease: I. A. Richards, Roland Barthes, Jacques Derrida, and Stephen Greenblatt. These generous readings are coupled with a decidedly upbeat view of the future of the public intellectual, whose widely broadcast obituary Dickstein declares much too premature. He even discerns signs that his postmodernist colleagues—having secured the English department lounge—are ready to stop using nouns like "foreground" as verbs and tackle the public sphere.

Although Dickstein's optimism on this score is bracing, I found him more persuasive when he was on the attack, especially in the guise of a curmudgeonly "Left Arnoldian" of "more partisan and intemperate" views than his own, whose imagined dialogue with a younger, more fashionable critic concludes the book. Here in defense of a "nondogmatic historicism" he lets fly with a spirited assault on the "herd of independent minds" among "new historicists" who have stripped the marching orders of Commander Greenblatt of their nuances and are "turning the study of history into the academic game that literature has become" (p. 173). These self-described "radicals," he complains, have "taken the utopian dream of social justice and equality that people have spilled blood over, and turned it into a professional method, squeezing the sap of life out of it" (p. 171). This is the compelling voice of a double agent who recognizes that it is not an auspicious time to come in from the cold.

ROBERT WESTBROOK
University of Rochester

ANN HULBERT. *The Interior Castle: The Art and Life of Jean Stafford.* New York: Alfred A. Knopf. 1992. Pp. xv, 430. $25.00.

Ann Hulbert intended in this work to "pursue the mystery of Stafford's literary personality," her "interior castle," through her writing. While no biographer can ever really know her subject, Hulbert creatively explores this famous mid-twentieth-century American writer's "best kept secret." In the complex, detailed analysis of Stafford's writing, Hulbert skillfully intertwines even the smallest details of the writer's life to explore convincingly Stafford's "complicated creative identity" (p. xi).

Born in Southern California, Stafford moved to Colorado as a youngster and showed an early interest in writing. Her rocky, resentful relationship with her parents, particularly her father (an unsuccessful writer), never diminished as Stafford grew into adulthood. This "sense of homelessness," of "feeling unwanted," of "marginality" (pp. 208, 235, 356) and hostility toward her father became themes Stafford returned to repeatedly in her work.

She married the poet Robert Lowell in 1940, several years after graduating from the University of Colorado. Literary and commercial success—completing two of her three novels, *Boston Adventure* (1944) and *The Mountain Lion* (1947)—emotional highs and lows, and wrestling with Catholicism characterized Stafford's years in this relationship. She also began to suffer from what became lifelong bouts with illness and heavy drinking, periods of intense desire to satisfy domestic needs, and distancing from close friends. The couple moved frequently, but no matter where they lived, their lives always intersected with literary greats including Allen Tate and Robert Penn

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Warren, and their contemporary writer friends such as John Berryman and Randall Jarrell. When the Stafford-Lowell relationship finally ended in 1946, with both close to mental breakdown, Stafford sought refuge in a psychiatric hospital, despite her fear that "freedom from her demons . . . might destroy or dilute her creative gift" (p. 227).

After leaving the hospital, she sought a new community, which she found at *The New Yorker*. She published her third novel, *The Catherine Wheel*, in 1952 plus numerous short stories, and still received critical recognition. This attention pleased her but never erased identity questions. When she married the noted writer A. J. Liebling, also with *The New Yorker*, in 1959, Stafford's productivity essentially ended, although she continued to write short pieces. She received the Pulitzer Prize in 1970 for *The Collected Stories of Jean Stafford*, recognizing past rather than recent work. Although she never considered herself a feminist, she acknowledged the autobiographical nature of her work as a woman writer as well as the oppressive nature of many men in her life.

Hulbert had the good fortune of having available numerous rich letters between Stafford and her friends, both male and female, plus interviews, all of which she mined meticulously. The extensive correspondence with Lowell proved particularly helpful in documenting the lifetime dependence and tension between the two writers, a major theme in the book. Hulbert weaves together Stafford's public and private lives as a seamless whole, which is not an easy task, especially given Stafford's tendency to hide her inner self. Stafford herself acknowledged how her life not only influenced her work but also, perhaps even more importantly, how her writing influenced her life, both her public success and at the "private, psychological level" (p. 228). Hulbert interprets these connections with skill. But her determination to document thoroughly and to integrate many references to Stafford's considerable writings sometimes results in confusing narrative. It is also puzzling why Hulbert strives to hide her own biases as she searches for the hidden Stafford. Overall, however, Hulbert does an excellent job of providing new information on and interpretations about the considerable literature on Stafford, and in the process adds important dimensions to our understanding of many leading mid-twentieth-century literary figures.

INGRID WINTHER SCOBIE
Texas Woman's University

JOEL SPRING. *Images of American Life: A History of Ideological Management in Schools, Movies, Radio, and Television.* (SUNY Series, Education and Culture: Critical Factors in the Formation of Character and Community in American Life.) Albany: State University of New York Press. 1992. Pp. ix, 306.

This book is a solidly researched, ploddingly written exploration of the history of ideological management in schools, radio, television, and movies. Joel Spring defines ideological management as both propaganda—the activities that government officials engage "in manipulating information for the purpose of ideological control" (p. 4)—and as something more complex and inclusive, which involves the competition and conflict of a wide range of public and private groups to convey ideas and information through the schools and mass media. According to the author, this process has usually been dominated by government, private enterprise, and advocacy groups (for example, the American Legion, the National Council of Women, and the National Association for the Advancement of Colored People [NAACP]), who have continually struggled and negotiated with the media over what sort of images of American life should be presented to the public.

The book is structured topically. Spring depicts the Committee of Public Information's attempt to centralize control over public opinion during World War I by spreading a prowar message through poster campaigns, promoting good citizenship and patriotism in the schools, and using Hollywood to make propaganda movies. He then analyzes the struggle between advertisers and educators over the control of radio programming in the 1930s. Spring also examines how government concern over juvenile delinquency in the 1950s led to a comic book code and the sanitizing of television violence. He concludes by exploring how, in the early 1990s, some advocacy groups promoted multiculturalism while others, on the religious Right, sought to purge both immorality in television programming and secular humanism from the schools.

Parts of Spring's book merely rehash well-worn analyses, for example, of the effects of the repressive movie code of 1934, and the purge during the late 1940s and early 1950s of leftists and putative leftists from the film industry that Robert Sklar (*Movie-Made America* [1975]) and countless others have done more incisively. But if Spring is only able to paraphrase Todd Gitlin's trenchant critique of how the media portrayed Students for a Democratic Society and the antiwar movement in the 1960s, he does succeed, in a lucid though stolid fashion, in providing a great deal of useful information on the struggle over shaping the content of social studies textbooks.

The book is especially illuminating when dealing with how textbook images of family life, under pressure from the NAACP and Congress on Racial Equality, shifted in the 1960s from the cheerful, lily-white, middle-class families of the mythic "Textbook Town" to books with more integrated, urban, multiethnic images. Yet no real transformation occurred. The images that dominated these books envisioned African Americans playing harmoniously with whites on clean city streets, without ever allowing a hint of urban violence or squalor to disrupt this constructed

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American idyll. Spring extends this insight in his balanced discussion of *Sesame Street*, which he sees as both opening "the door . . . to the influence of national educational policy on the world of television" (p. 248) and sweetening and distorting urban realities.

This study avoids being intellectually reductive. Spring makes us aware that the power of the media is limited, that a gap exists between the images of American society projected by the media and the attitudes and behavior of the public. But he never really explores all the ramifications of this complex relationship. He has written a sound book, but an uninspired one.

LEONARD QUART
College of Staten Island, City University of New York

GREG MITCHELL. *The Campaign of the Century: Upton Sinclair's Race for Governor of California and the Birth of Media Politics*. New York: Random House. 1992. Pp. xx, 665. $27.50.

It is one of those incidents that is mentioned in every book on the Great Depression, usually in connection with the panacea movements of the early 1930s. In 1934, Upton Sinclair, the muckraking author and former socialist, nearly became governor of California on the basis of a visionary economic plan that he called EPIC, short for End Poverty in California. Calling for the state to take over idle factories and farmland and set up cooperatives for the unemployed, his program of "production for use" became the basis for an extraordinary political movement that triumphed in the Democratic primary election and seemed for a time destined to win the statehouse, the legislature, and perhaps spread eastward. It did not happen. An ingenious conservative counteract, featuring the latest in media tactics, stopped Sinclair and the EPIC movement and saved California for the Republican Party.

Greg Mitchell's study is the first book-length treatment of the episode. Mitchell tells only part of the story, picking up events with Sinclair's primary election victory in August 1934 and following the campaign day by day through the November 6 general election. If that sounds limited, it is not. A skilled narrator with a talent for vivid description and entertaining anecdote, Mitchell uses the calendar to build a story that ranges very widely, from California to Washington and even to Berlin, while bringing together an amazing cast of famous personalities. Dorothy Parker, William Randolph Hearst, Charlie Chaplin, Eleanor Roosevelt, A. P. Giannini, Will Rogers, Louis B. Mayer, Charles Lindbergh, and dozens of other notables from the worlds of politics and media flit in and out of this narrative, as Mitchell tries to establish the interconnectedness of people and events at what he sees as a critical historical juncture.

The conjunction of media and politics in the campaign to stop EPIC made this a pivotal moment in American electoral politics, says Mitchell. Republican governor Frank Merriam owed his reelection to a powerful alliance of media moguls and business executives who created a new kind of campaign apparatus dominated by professional political consultants, who in turn used film, radio, and direct mail as they had never been used before. Mailings targeted select groups with brochures demonstrating Sinclair's dangerous ideas. Radio programs, including specially commissioned soap opera serials, broadcast scary impressions of what the socialist author would do to the jobs and liberties of Californians. And, most spectacular, MGM studios created and distributed faked anti-EPIC newsreels to movie theaters around the state.

This is not an altogether new treatment. Others have painted the same basic picture in broader strokes, but no one had dug through the archives and come up with the intimate details. And intimate is just the right word. The combination of stunningly effective research and omniscient journalistic voice takes this book deep into the private centers of the campaign, deeper than historians who worry about authority and evidence may like. Other matters of perspective can also be criticized. More interested in exposing than explaining, Mitchell makes little effort to investigate the motives of those allied against Sinclair, usually taking their conservatism for granted. Paying little attention to the subtleties of political thought, he thus misses the opportunity to track the kaleidoscopic changes in political commitments taking place in the early 1930s.

And what of the book's principal claim, that the California race was "the campaign of the century"? The argument is incomplete. Most of what Mitchell emphasizes was not particularly new, certainly not the strategy of destroying the opponent while ignoring the colorless client candidate, nor the use of professional consultants or radio in campaigns. And Mitchell fails to establish linkages coming out of the California campaign. Did media campaigning increase because of the EPIC experience or was it increasing anyway? And what about Hollywood's role? It is not clear that the studios' stunning entry into electoral politics was ever repeated in the same fashion. Perhaps the campaign of 1934 was as much a cautionary experience as the wave of the future. Unfortunately, Mitchell assesses the campaign's impact in only cursory fashion, thus neglecting to defend the book's bold title. The fault to some extent lies in the chronological structure of the book, which leaves little room for developed argument. But that structure is also the book's strength. Mitchell has created an engaging narrative that brings to life a dimly understood political confrontation. It may not have been the campaign of the century, but Mitchell has made

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sure that Sinclair's EPIC campaign will now be treated as one of the critical episodes of the 1930s.

JAMES N. GREGORY
University of Washington

RICHARD A. REIMAN. *The New Deal and American Youth: Ideas and Ideals in a Depression Decade.* Athens: University of Georgia Press. 1992. Pp. viii, 253. $35.00.

The New Deal created two major agencies for youth, the Civilian Conservation Corps (CCC) in 1933 and the National Youth Administration (NYA) in 1935. Yet in recent years, when Americans remember the 1930s and politicians cast about for ways to aid deprived youth, most recall only the forest armies of the CCC. Richard A. Reiman recounts the hopes and ambitions of the people who planned and led the lesser known NYA.

Reiman decries historians' summaries of the NYA as merely a junior Works Progress Administration or student aid plan, and he challenges overemphasis on Eleanor Roosevelt's initiative in its creation. Drawing on correspondence and agency records, Reiman tells a complex story of bureaucratic infighting and rival plans, all governed ultimately by Franklin Roosevelt's political calculations. Under pressure from college presidents and student leaders, Roosevelt's administration funded 75,000 part-time jobs for college students. Reiman describes the NYA as emerging from the struggle that followed during 1934 and 1935: the U.S. Office of Education, allied with professional educators, favored expanding student aid with minimal federal control. A second group, whom Reiman credits with broader vision, centered on Harry Hopkins, Charles Taussig, and Aubrey Williams. Convinced that the Depression had disrupted socialization of the young, this group campaigned for vocational training and education in democracy. More than Reiman wants to admit, Roosevelt compromised: Williams would head the NYA, but the majority of funding flowed to student aid and work relief. Reiman virtually ignores those functions of the NYA and focuses instead on Williams's creation of residential centers where isolated rural youth would learn industrial skills and group democracy. After 1938, however, Roosevelt's concerns shifted toward preparedness. In Reiman's view, Williams sacrificed principle by reorienting the NYA to train workers for war production; he won big appropriations, but the NYA succumbed to Congressional conservatism by 1943.

Reiman's work supports the interpretation of the New Deal as politically guided improvisation, deficient in "long-term vision" (p. 183). In branding as conservative those educators who favored aid to college students, Reiman leaves open the question whether Williams's and Taussig's promotion of trades training offered any broader vision of advancement for lower-class youths.

This book may interest students of New Deal policy formation but offers little to social historians of youth. Reiman neglects the work of John Modell and others on transitions to adulthood. He seldom describes NYA programs in action and dismisses as "arid" any attempt to judge their results: "The question that is historically answerable ... is not what the NYA accomplished but what its architects sought to accomplish" (p. 180). The result is a history which pays scant attention to NYA programs such as student aid that touched many young people and concentrates instead on plans and proposals that went largely unrealized. The book also suffers from awkward writing. For an understanding of New Deal youth programs, Reiman's work supplements but does not displace John Salmond's more graceful accounts, *The Civilian Conservation Corps* (1967) and his biography of Aubrey Williams, *A Southern Rebel* (1983).

DAVID MACLEOD
Central Michigan University

DOUGLAS CARL ABRAMS. *Conservative Constraints: North Carolina and the New Deal.* (Twentieth-Century America Series.) Jackson: University Press of Mississippi. 1992. Pp. xx, 285. $39.95.

According to Douglas Carl Abrams, Depression-era North Carolina provides a classic example of how "entrenched conservatism could successfully constrain reform" (p. xvi) at the state level. The New Deal's limitations and the strength of its opposition has become a well-established theme in New Deal historiography, and Abrams has thoroughly demonstrated it for North Carolina. His work complements such outstanding studies as Anthony Badger's *Prosperity Road: The New Deal, Tobacco, and North Carolina* (1980) and Ronald L. Heinemann's *Depression and New Deal in Virginia: The Enduring Dominion* (1983).

Abrams shows that despite its reputation for progressivism, North Carolina was dominated by a business-minded conservative establishment. Its key figure was O. Max Gardner, governor from 1929 to 1933 and a strong influence throughout the 1930s. Gardner laid down a Herbert Hoover–like agenda of property tax reduction, retrenchment, and budget balancing that was followed by his successors, J. C. B. Ehringhaus (1933–37) and Clyde Hoey (1937–41). Yet this establishment was not openly reactionary; some of the conservatives' success was due to their willingness to accept parts of the New Deal and their rhetorical support of a popular president. Most of the North Carolinians in Congress followed a similar pattern of qualified support, although a few, like Senator Josiah Bailey, were among the staunchest anti–New Dealers in Washington. Abrams finds North Carolina liberals ineffective because of weak candidates, an unimaginative grasp of issues, and the fact that the ablest progressives (Josephus Daniels, his

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son Jonathan, and Frank Graham, for example) were not at the center of political action.

Throughout the book Abrams focuses on how New Deal programs functioned at the grass roots and how North Carolinians supported or obstructed them. Not surprisingly, he finds that conservatives favored measures that reinforced their own interests. Thus, textile executives welcomed—in fact, wrote—a code for the National Recovery Administration (NRA) to stabilize their severely depressed industry, whereas the consistently profitable cigarette companies had little use for a code and did not agree on one until shortly before the demise of the NRA. Similarly, tobacco farmers supported the Agricultural Adjustment Administration (AAA), which was highly successful in raising their prices.

New Deal programs that had potential for far-reaching reform were so restricted by conservative opposition or indifference that Abrams concludes it was "remarkable . . . how little North Carolina's society, economy, and politics had changed by the end of the decade" (p. xvi). Business leaders whose own interests had been satisfied developed no enthusiasm for improved labor standards, just as farmers with AAA tobacco allotments rarely looked beyond parity prices to the problems of rural poverty. Opposition from the Duke power interests limited the work of the Rural Electrification Administration. The state made no contribution to unemployment relief, and the legislature was one of the last in the country to initiate participation in several key Social Security programs. Local support was lacking for Works Progress Administration projects (in which the state ranked last) and even for the generally popular Civilian Conservation Corps. In a chapter on black North Carolinians, Abrams finds that they benefited minimally from New Deal programs, which, overall, affected them "more adversely than positively" (p. 183).

Abrams's painstaking research is evident from his thorough bibliographical essay and also from useful historiographical comments, which he skillfully weaves into his narrative. Although he presents no startling new interpretations, Abrams has increased our understanding of the New Deal at the state level.

Paul E. Mertz
University of Wisconsin, Stevens Point

Thomas Biolsi. *Organizing the Lakota: The Political Economy of the New Deal on the Pine Ridge and Rosebud Reservations*. Tucson: University of Arizona Press. 1992. Pp. xxii, 244. $35.00.

Anthropologist Thomas Biolsi has written a fascinating, perceptive, and provocative analysis of the impact of the federal reform policies of the Indian New Deal (1933–45) on the political structure and processes of the Lakota tribes (Western Sioux or Teton Sioux speaking the Lakota dialect) on the Pine Ridge and Rosebud reservations in South Dakota. To a greater extent than previous case studies of the implementation of the Indian Reorganization Act (IRA) of 1934, the major initiative of Indian Commissioner John Collier during that period, Biolsi's work demonstrates how the federal government lured tribal people into considering acceptance of a flawed, non-Indian model of self-government that disregarded the reality and status quo of reservation politics and culture.

Biolsi approaches his topic from a Marxist perspective that is attentive particularly to how structural political and economic power circumscribed social behavior. The power in this case was held historically by the federal bureaucracy, most visibly in the form of the Office of Indian Affairs (OIA)—which became the Bureau of Indian Affairs officially in 1947—and was maintained through what the author describes as "its remarkable technologies of surveillance and control" (p. xxi). Although the New Deal reformers held out to tribes the promise of empowerment to seize control of reservation resources and administration if they would organize their political structures according to a federal model, Biolsi demonstrates that their rhetoric was not matched by either the language or implementation of the IRA. Rather than transferring power and establishing tribal self-determination and autonomy, the IRA, Biolsi demonstrates, had the effect of creating an additional, superfluous level of government over the Lakota and establishing new levels of federal domination.

In describing the range of political possibilities that the Lakota saw before them during this era, Biolsi focuses on the complex nature of their continued resignation to OIA domination. He shows that the crisis of authority brought about by the creation of the ineffectual IRA-sanctioned tribal councils sparked resentment and resistance among the Lakota, which eventually manifested itself in the armed occupation of the Wounded Knee hamlet on Pine Ridge in 1973. He also ponders the questions of why this resistance focused on their own tribal governments rather than on the OIA and why, indeed, the Sioux never considered life without the federal bureaucracy to be within the realm of political or economic possibility.

The primary significance of this book in comparison to the fine case studies by historians Donald L. Parman, Laurence M. Hauptman, and Harry A. Kersey, Jr. (on the impact of the Indian New Deal on the Navajo, Iroquois, and Florida Seminole respectively) is that it is the first study to investigate tribes which voted to accept the IRA rather than reject its provisions for self-government. Based on an impressive balance of extensive archival sources and field interviews, this volume makes an extremely valuable contribution to our understanding of both the grass-

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roots implementation of federal Indian policy and the evolution of modern tribal governments.

MICHAEL L. LAWSON
Historical Research Associates
Annandale, Virginia

WILLIAM J. GRIMSHAW. *Bitter Fruit: Black Politics and the Chicago Machine, 1931–1991*. Chicago: University of Chicago Press. 1992. Pp. xiv, 248. $24.95.

William J. Grimshaw's study of the relationship between African Americans and the Chicago Democratic machine provides a comprehensive and analytical view of an enduringly interesting and significant problem. In terms of both methodology and content, it is a major contribution to the specific problem and to its national relevance.

Grimshaw begins with two chapters on methodology, analyzing traditional models of explanation and the most influential prior interpretations, especially those of Harold F. Gosnell and James Q. Wilson. This is valuable for specialists in the field and provides a context for the remainder of the book. I do think that what he calls the "economic" and "sociological" perspectives are not so discrete as he makes them, and that the role of culture is understated here (its importance is clearly recognized later in the book); also, his coverage of the work of Gosnell and Wilson becomes somewhat excessive. Nonetheless, this section is important.

The remainder of the book is roughly chronological, but always analytical. Grimshaw explains the black shift to the Democratic Party in the late 1930s as the result of a combination of factors: the Depression, New Deal programs, and the activities of the local machine. With this came a black machine elite, led by William Dawson. But, as the author correctly notes, Chicago's blacks gained little advantage and less real power during this period of voting change.

The rise, development, and decline of the Richard J. Daley machine, and its relationship to black voters, is the book's major focus. This was the period of almost total black support of the machine and the Democratic Party. Indeed, the Daley machine could not have existed without its massive support from blacks, and, given how little it actually served the needs of, or granted influence to, Chicago's African-American community, it is almost surprising that the machine avoided rupture as long as it did.

Grimshaw does a particularly fine job of relating the rise of civil rights (the cultural forces alluded to above) to the relationship between blacks and the machine. Here blind loyalty was replaced by new leadership and greater demands; the beginnings of real black empowerment took shape. Racial politics became a central focus of Chicago politics. Grimshaw's tables and text provide a convincing analysis of the black role in the Daley machine's decline, but a more extensive analysis of voter turnout would have been helpful here.

Grimshaw continues his analysis through the interim years of mayors Jane Byrne and Michael Bilandic. This was a time of continued machine decline and even more contentious racial politics, culminating in the rise of Harold Washington, Chicago's first black mayor, and then the coming of Richard M. Daley and a quite different Chicago machine.

This is an inadequate description of a very rich book, full of ideas, data, and interpretation. Grimshaw's role as scholar-activist does come through, particularly in his discussion of Washington, but his analysis is always cogent and defensible. The documentation is meager, but Grimshaw has read widely and the facts he is dealing with are less enigmatic than their meaning.

Overall, the book is a major contribution to both African-American history and the history of urban politics. It has much for the expert to ponder, and is written in a way that makes it entirely accessible to the more general reader as well.

JOHN M. ALLSWANG
California State University, Los Angeles

STANLEY SANDLER. *Segregated Skies: All-Black Combat Squadrons of World War II*. (Smithsonian History of Aviation Series.) Washington, D.C.: Smithsonian Institution. 1992. Pp. xv, 217. $24.95.

This is a potentially outstanding, much-anticipated study of African-American air combat units in World War II. It deals with an interesting and important subject, and Stanley Sandler has examined a wide array of primary sources and secondary works. Moreover, the literature of World War II lacks a well-documented, comprehensive study of the origins, establishment, training, combat operations, and dissolution of these segregated units. Unfortunately, the book does not live up to its potential.

The story of America's black combat pilots in World War II is a compelling one. Until 1941, African Americans were excluded from the Army Air Corps, predecessor to the Army Air Forces (AAF) and the U.S. Air Force (USAF). After several years of intense lobbying by the black press and civil rights leaders, the Air Corps reluctantly admitted blacks, but only in segregated units. In March 1942, the first black pilots completed flight training at Tuskegee Army Air Field, Alabama. By the end of the war almost 1,000 African Americans had qualified as AAF pilots, and about half flew combat in the Mediterranean and European theaters. Although handicapped by segregation and racism, these all-black air units performed effectively and compiled respectable combat records. Remnants of the units survived until 1948, when the USAF began to desegregate.

Although the breadth of Sandler's research is im-

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pressive, the study suffers from documentation problems. Accepted practices of scholarly citation are abandoned in favor of a cumbersome system of endnotes keyed to page numbers and phrases within the text. Of more concern are the glaring gaps in documentation. Reconstructing air battles is exceedingly difficult because of the speed and three-dimensional nature of air combat. Yet Sandler recounts numerous aerial engagements in great detail, complete with extended quotations. Unfortunately, these segments contain virtually no documentation, not even for the quotations (see, for example, pp. 55–58, 110–13). The publisher's disclaimer following the title page offers little reassurance: "The descriptions of otherwise undocumented personal incidents and the recollections of episodes and persons are entirely the author's."

Moreover, the text contains a number of nagging errors and inconsistencies. For example, Sandler incorrectly identifies Texas governor Coke Stevenson as an Alabama senator (p. 36). He credits black airmen with the destruction of 552 enemy aircraft (p. 158), yet his appendix (extracted from an authoritative USAF study) credits them with just over 100. Finally, the introduction affirms the oft-cited claim that black fighter pilots "never lost a bomber to enemy aircraft, a record unique in U.S. Air Force history" (p. vii). Unfortunately, the text does not assess the accuracy and significance of this claim but instead seems to contradict it by concluding that the loss of bombers escorted by the black airmen was "very small" (p. 116).

Despite its shortcomings, this book is nevertheless a welcome addition to the growing body of literature on the black experience in the U.S. military. The scope of Sandler's research is broad and opens many new avenues of inquiry for further research on this important phase of aviation history.

ROBERT J. JAKEMAN
Auburn University

MARGARET A. BLANCHARD. *Revolutionary Sparks: Freedom of Expression in Modern America.* New York: Oxford University Press. 1992. Pp. xiii, 572. $49.95.

Margaret A. Blanchard has set out to write a general history of free speech in modern America. No such book exists, and merely to establish the parameters for such a survey is a useful project in itself. The comprehensiveness of the endeavor is its greatest strength. From labor organizing and wartime censorship to the civil rights movement and the sexually explicit photographs of Robert Mapplethorpe, Blanchard covers just about every area in which issues of free speech arise.

The book aims at methodological inclusiveness as well. Blanchard is trying to transcend the traditionally juridical approach to freedom of expression and to place it within a broader social and political context. She does not always succeed. A project of this scope necessarily requires a near-encyclopedic grasp of over a hundred years of American social and political history, and there are places where Blanchard, a professor of mass communications, simply lacks the background. Thus, for example, her discussion of Anthony Comstock's late-nineteenth-century drive for sexual purification remains strangely innocent of the important body of work on Victorian sexuality.

Even where she has read the appropriate books, she has not always used them correctly. The breadth of Blanchard's inquiry makes it difficult to assess how well she handles topics about which I know little. When she deals with the 1940s and 1950s, however, her account is full of errors, both factual and interpretive. Some, like identifying Korea as a "Southeast Asian country" (p. 248), may just be sloppiness. But others reveal a serious lack of historical background and a distressing proclivity for invention. She claims, for example, that the House Committee on Un-American Activities became a permanent committee because of the red scare when, in fact, it achieved that status as a result of a parliamentary maneuver. Another fabrication is the unsupported statement that Klaus Fuchs named Julius Rosenberg and Ethel Rosenberg as atomic spies. Blanchard also leaves things out. For example, in discussing the collapse of the American Communist Party, she fails even to mention Nikita Khrushchev's revelations in 1956 about Joseph Stalin's crimes, just as in assessing the political demise of Senator Joseph McCarthy, she overlooks the Army-McCarthy hearings. Where Blanchard came by these errors and misinterpretations (there are many more) is a mystery; they are not in the scholarship she cites.

Perhaps the other sections of the book bear a closer relationship with reality. Blanchard's treatment of the relationship between the economic structure of the newspaper industry and its failure to confront the political establishment seems well informed, as does her discussion of the attempt to censor pop music lyrics and other recent challenges to free expression. Whether accurate or not, however, these sections seem as confused as the rest of the book, for the narrative hops from one topic to another without trying to tie them together or offering an overall analysis. Surely an ambitious project like this should present some kind of explanation for the nation's changing patterns of dissent and repression. But Blanchard only ventures the tautological notion that the course of free speech is cyclical, waxing and waning with the tolerance for dissent of the government and the American people.

ELLEN SCHRECKER
Yeshiva University

WILLIAM B. HELMREICH. *Against All Odds: Holocaust Survivors and the Successful Lives They Made in America.* New York: Simon and Schuster. 1992. Pp. 348. $23.00.

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There are many questions about the Holocaust that remain unanswered even after approximately three decades of intensive research. William B. Helmreich's study, a comprehensive analysis of the survivors who came to the United States and the lives they have built, is a welcome addition to this field. In addition to interviewing close to 200 survivors, he conducted extensive random surveys. He demonstrates that those who experienced some of the most horrible treatment humans have ever faced are not chronically depressed, anxious, and fearful.

Despite the fact that many survivors saw their families killed and knew that they might soon be candidates for the gas chambers, they were able to reconstruct their lives in the post-Holocaust years. They not only rebuilt their lives but did so in a constructive and, given their experiences, remarkable fashion. The many who have raised families, achieved professional and economic success, and made organizational and financial contributions to their communities constitutes strong evidence that these people have not allowed their suffering to impede their ability to function in society. Although Helmreich acknowledges that there are those among the survivors who had difficultly building a "normal" life, his focus is clearly on the most stalwart among them. At times Helmreich seems so overwhelmed by the accomplishments of the survivors that he glosses over the more problematic areas. The relationship between survivors and their children is often far more complex than he seems to acknowledge. And what about survivors who have not made professional and economic strides? Are they to be considered failures?

But the survivors' ability to reconstruct their lives is Helmreich's main focus. How were they able to do so? What particular personality traits enabled them to do so? According to Helmreich, the traits that enabled survivors to rebuild their lives included flexibility, assertiveness, tenacity, optimism, intelligence, distancing ability, group consciousness, finding meaning in one's life, and courage. It is, of course, hard to imagine that anyone possessing such qualities would not succeed in building a life in America, despite the horrors of their previous experiences. These people were, after all, survivors even before they reached these shores. His desire to celebrate the survivors' formidable achievements and minimize some of the other aspects of their experiences may result from the fact that his work is a corrective to so many of the earlier studies on survivors and their children, which focused exclusively on their psychological and emotional problems.

Ultimately, this book demonstrates that people can overcome the most formidable odds and maintain a lifelong capacity for change. It also reminds us that while "survivor" has become the appellation by which so many of these people are known, they have dramatically different personalities and histories. As one survivor observed, "I was not born a refugee. I was not born an orphan, and I was not born poverty-stricken. I was a loved child that had a family and parents and belonged" (p. 151).

Helmreich's book illustrates that this is a fertile field for research. It may not, however, become part of a larger body of literature because the survivors are aging and the opportunity to do primary research among them is rapidly fading.

DEBORAH E. LIPSTADT
Emory University

JAMES N. GIGLIO. *The Presidency of John F. Kennedy.* (American Presidency Series.) Lawrence: University Press of Kansas. 1991. Pp. x, 334.

This concise account of the brief presidency of John F. Kennedy by James N. Giglio fully lives up to the promise of the editors of the American Presidency Series that its volumes will be "interesting" as well as "scholarly" (p. vii). It is more than merely interesting; its narrations of the major crises of the Kennedy presidency—the Bay of Pigs, civil rights, the Cuban missile emergency—are riveting. The reader relives the missile crisis, for example, as if in the same room with the ad hoc group known as ExComm as they debated the alternative courses of action from which the president ultimately chose. Yet the chapter can still present the crisis as seen through Nikita Khrushchev's eyes as well as Kennedy's and critique the provocative behavior on both sides that preceded the confrontation.

Giglio's fascination with what was dramatic and exciting in 1961–63 leads him to devote more than twice as much space to foreign as to domestic policy (excluding civil rights), and while this may fairly reflect journalistic and public interest at the time, it may not truly measure the relative historical importance of the various governmental initiatives of the Kennedy years. Within the domestic area, civil rights is accorded a full chapter, while, for instance, one paragraph is devoted to housing, four to medicare, and five to education. Barely four pages are given to the centerpiece of Kennedy's fiscal policy, his proposed general tax cut, while six are devoted to farm commodity programs. Neither the Appalachian regional development program nor environmental issues are mentioned.

Organizing history by separate volumes on individual presidencies has inevitable limitations. To describe any Kennedy domestic program requires arbitrarily extracting three years' events from a stream of history that may have begun with Franklin Roosevelt and culminated with Lyndon Johnson. On occasion, the author flashes backward or forward, but he cannot do so at enough length to put the Kennedy years in perspective. Two distorted impressions follow. One is that, despite a general disclaimer (p. 97), Kennedy's proposals had no earlier history, that they sprang full-blown from the White House in 1961 rather than from an agenda rather fully developed as

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a Democratic Party program during the Dwight Eisenhower administration. The other distorted impression is that the presidency is all-important as initiator and executor, while the Congress participates primarily as an obstruction, and the bureaucracy is either a nuisance or irrelevant.

Some of the many aspects of the presidency are not discussed at all. What of Kennedy as administrator; were there no innovations in government management? What of Kennedy as party leader; what happened to the Democratic Party during his tenure? Perhaps nothing consequential occurred in either case, but that might at least be said and explained. Disappointingly, Giglio does not draw at all on the considerable body of political science literature analyzing the presidency as an institution, tracing its evolution and comparing the behavior and performance of its occupants, particularly recent ones, in the various presidential roles.

Yet Giglio deals at satisfying length, and authoritatively, with three controversial questions: Kennedy's health, his extramarital affairs, and his assassination. On the first two of these subjects, the author supplemented the information available at the Kennedy library and in published works with valuable additional material gained through interviews and correspondence with informed sources.

One book cannot cover everything a president does, or should do, and still achieve the aim of being interesting, and there is always the *Congressional Quarterly Almanac* for those who would dig deeper into program issues. On those subjects Giglio chose to emphasize, he researched and wrote exceedingly well.

James L. Sundquist
Brookings Institution

Barbie Zelizer. *Covering the Body: The Kennedy Assassination, the Media, and the Shaping of Collective Memory.* Chicago: University of Chicago Press. 1992. Pp. viii, 299. $29.95.

Barbie Zelizer's study is about journalists telling stories—stories not so much about the assassination of John F. Kennedy as about themselves. Zelizer does not settle the endless dispute over what actually happened in Dallas on November 22, 1963. Yet she clearly reveals how journalists established themselves as the "preferred interpreters" of Kennedy's death and how, in the process, they redefined journalism itself.

In the first three chapters, Zelizer provides a theoretical framework for understanding journalism as a cultural institution. Reviewing a broad literature on narrative, rhetorical legitimation, and cultural authority, Zelizer establishes the Kennedy assassination as a "critical incident" in the history of journalism, an event that combined with the advent of television and other developments to redefine what it meant to be a journalist. In chapters 4 through 6, Zelizer examines the original coverage of the Kennedy assassination, demonstrating how journalists transformed stories grounded in hearsay and rumor into tales of professional triumph. In chapters 7 and 8, Zelizer traces how journalists in later years promoted themselves over other sources—politicians, historians, and independent critics—as the "preferred retellers" of the Kennedy assassination. In the process of "retelling" the story, as Zelizer shows in chapters 9 through 11, journalists established today's hierarchy of celebrity reporters and prestigious news organizations. In recent years, journalists have even usurped the role of professional historians, declaring their word the "final word" on Kennedy's death.

In an epilogue inspired by Oliver Stone's *JFK* (1991), Zelizer considers the film as a serious new challenge to journalistic authority. Not surprisingly, journalists went to great lengths to discredit Stone's film. Zelizer's explanation sounds an ironic note: journalists so vehemently denounced Stone's "fictionalized portrayals" because the filmmaker's methods so closely "paralleled their own practices" (p. 212).

Zelizer's critique of contemporary journalism is far-reaching and devastating. The journalists' own words do most of the damage, as throughout the book we witness them shamelessly exploiting Kennedy's death to promote themselves. Zelizer's analysis also provides further documentation of more general trends that have troubled a number of media critics in recent years: the growing "tabloid" mentality among even mainstream journalists, for example, and the unbridled arrogance of television's "star" reporters.

Zelizer borrows from the politically charged scholarship of contemporary cultural studies and rhetorical theory. Yet unlike many of her sources, Zelizer refrains from ideological posturing, and her book is comparatively free of theoretical jargon and obfuscation. Like many media critics, she also blames the poverty of modern journalism on an "uncritical and inattentive public" (p. 197). Although one might dispute whether the excesses of journalists can be blamed on the public, this book remains an important contribution. In meticulous detail, Zelizer documents three decades of self-congratulatory myth-making by journalism's elite and shows how those myths have shaped and reshaped our memories of Kennedy. Above all, Zelizer debunks journalism's claim to objectivity, and in doing so she performs a valuable public service.

J. Michael Hogan
Indiana University, Bloomington

Marty Jezer. *Abbie Hoffman: American Rebel.* New Brunswick, N.J.: Rutgers University Press. 1992. Pp. xviii, 345. $22.95.

Marty Jezer, an activist of the 1960s who then wrote numerous insightful articles in the underground

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press, now has written a biography of Abbie Hoffman. The result is an enjoyable book that some former activists are sure to read with pleasure, for as the author admits, "I loved Abbie for his courage and audacity, his humor, and his imagination." To Jezer, Hoffman was "a heroic figure," one of the "best grassroots, seat-of-the-pants community organizers this country has likely ever known" (pp. xiii–xv).

This is a favorable biography, which also is a problem with the book. The author could have spent more time on those actions of Hoffman's in the late 1960s that often enraged many serious activists who felt his comic behavior was simply beside the point, which was, of course, changing the domestic and foreign policies of the United States. Furthermore, the portrait Jezer presents here is too narrow. This is not a sweeping painting of a man and his era, which constitutes the finest biographies. The author rarely discusses the dynamics within the fractured civil rights or antiwar movements, and he does not explore significant issues concerning the other causes of the era, from student power to women's liberation. Instead, this is Hoffman's story, a book about a small number of activists—the most vocal movement radicals—who were only a tiny fraction of the broad social movement of the 1960s.

Nevertheless, Jezer has produced a stimulating biography. The writing is energetic. The author has conducted extensive interviews, researched underground and establishment newspapers, and also taken fragments from Hoffman's various statements and writings to create a coherent story. This is an impressive accomplishment considering the subject, a manic-depressive personality.

What do we learn? This book informs us about Abbie's early years, his disagreements and fights with his father, his high school clowning, his intellectual stimulation at Brandeis University. Jezer is particularly good in describing Hoffman's work in the early 1960s with the Poor People's Corporation and his attraction to and participation in the hippie scene in New York's East Village. The author also makes a fine contribution by detailing the later period after the trial of the Chicago Seven, when Hoffman was arrested for selling cocaine, became a fugitive and environmentalist activist named Barry Freed, and then endured the frustrating years speaking on campuses during the conservative 1980s. Jezer reports that throughout his life Hoffman suffered psychologically and reveals that he had previously attempted suicide before taking his life in 1989.

Hoffman always was a man in search of a movement, and, as Jezer describes superbly, he was a rebel, a wise guy, a cheerleader, a man who rarely could compromise. Now that almost every book by and about radicals of the 1960s is out of print, Jezer's study is a welcome addition to the growing library on the 1960s.

TERRY ANDERSON
Texas A&M University

JOSEPH A. FRY. *John Tyler Morgan and the Search for Southern Autonomy*. Knoxville: University of Tennessee Press. 1992. Pp. xvi, 344. $34.95.

As a U.S. senator for thirty years, John Tyler Morgan of Alabama was often an irritant to presidents. Theodore Roosevelt once described him as "wholly indifferent to national honor or national welfare" (p. 247). Joseph A. Fry demonstrates in this excellent biography that Morgan's readiness to challenge the executive branch arose from his intense commitment to a southern nationalism that unified his political life. Fanatical about white supremacy in southern politics, Morgan sought to use foreign policy to free his region from perceived northern domination. Fry argues persuasively that Morgan's quest for southern autonomy explains his complex political career.

To track Morgan across so much of southern history was no easy task for Fry. In this lucid and well-researched narrative, he examines Morgan's place in Alabama politics with clarity and insight. The Populist revolt of the 1890s and the ferment stemming from it tested Morgan's ability as a factional leader. Fry shows how the senator played the race card on behalf of the status quo. Morgan emerges as a decisive architect of the conservative political system that so limited whites and blacks in Alabama after 1900.

Fry is equally deft in his treatment of Morgan in the Senate. Addicted to long-winded speeches, equipped with a photographic memory, and ready to outwork anyone else, Morgan gained an influence in foreign affairs that enabled him to push his sectional agenda on his senatorial colleagues and a series of presidents. Fry treats with evident skill the part Morgan played in the coming of the war with Spain, the annexation of Hawaii, and other episodes of American imperialism. Only William McKinley managed to work well with the prickly Morgan. The senator despised Grover Cleveland and distrusted Theodore Roosevelt.

Morgan's great dream was a canal across Nicaragua that would make Mobile a major seaport and free his region from northern economic control. Despite Morgan's legislative maneuvers, Roosevelt and the Panama Canal lobby blocked the Nicaraguan waterway between 1901 and 1904. Although Fry recognizes that Morgan's efforts were important in the development of canal policy, he also understands that the real power lay with John Hay and Roosevelt.

Morgan's campaign for Nicaragua failed, and his political career ended in anticlimax. The senator has now slipped into obscurity. In his day, however, he was a presence in national affairs. Fry's judicious and incisive treatment of this key southern senator reveals why Morgan exercised such influence during the Gilded Age. The poignancy of Morgan's life, as Fry shows so well, was that his campaign for southern autonomy destined him for historical irrelevance as

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the tenacious champion of a flawed and misdirected sectional agenda.

LEWIS L. GOULD
University of Texas, Austin

WARREN I. COHEN. *East Asian Art and American Culture: A Study in International Relations*. New York: Columbia University Press. 1992. Pp. xxi, 264. $32.50.

This pathbreaking book by Warren I. Cohen ventures into exotic territory once considered by historians of foreign relations to be as remote and intellectually uninhabitable as the Antarctic. In previous forays into unoccupied cultural ground, diplomatic historians have tended to confine themselves to the study of activities that have an obvious, although self-admittedly marginal, relevance to foreign policy: international education, exchanges, philanthropy, and so on. To the extent that art has been tackled at all, it has been treated as a propaganda side show put on by governments concerned to demonstrate the worthiness of their high culture. Cohen's work departs from this pattern by conceiving of art as an aspect of international relations that merits study in its own right.

It is a fascinating tale, with a classic story line. Following a preliminary period of exposure to Asian art, serious collectors began to emerge in the late nineteenth century. The "golden age" of collecting came in the period 1893–1919, with the building up of the first great collections in the Boston Museum of Fine Arts and the Freer Gallery in Washington. Following World War I, collecting was professionalized, as museum curators adopted more systematic approaches to evaluating and collecting art works. Yet further maturation occurred with the development, after World War II, of a significant community of historians of East Asian art, a phase in which exuberant growth gave way to pained assessment and conflicted judgment. The story is pushed along by Cohen's spry, often irreverent prose and by a series of entertaining character sketches of the varied lot of collectors, curators, and dealers who made possible America's accumulation of oriental artistic treasures.

The sordid episodes that punctuate the story make quite clear that the process whereby art works migrated across the Pacific owed more to power and imperialism than to culture. The ability of Americans to build stunning collections of Chinese and Japanese art was a function not only of America's rise as a world power but also of spells of weakness in which the Chinese and Japanese governments were powerless to prevent the draining away of national treasures. To that extent, the story of American culture's assimilation of East Asian art and the contribution that art made to America's growing consciousness of the Far East fits quite comfortably into our traditional power-dominated mode of understanding international relations.

The role of art as thus described is not inconsiderable, although Cohen's evaluation of art's direct impact on foreign policy and its success in promoting greater international understanding is quite restrained. Nevertheless, the discussion in his concluding chapter of art historians and their conflicting interpretive approaches suggests that art's foreign policy role is not restricted merely to reflecting sociopolitical realities. His discussion of the disputations between universalists and relativists, between contextualists and formalist connoisseurs, and between champions of professionalism versus high culture in Chinese painting ought to be required reading for all diplomatic historians for its demonstration of how debates in other academic fields are more balanced and varied conceptually than they are in diplomatic history, a discipline with strong natural affinities to intellectual history yet whose major interpretive schemes tend to be dominated by versions of contextualism. But beyond that, Cohen's treatment of art also forces one to think about how policy is made. If art is a metaphor for culture, and if international relations are defined as intercultural relations, then the study of how art is created and evaluated can tell us a great deal about how foreign policy is made and interpreted. Specifically, Cohen's discussion of artistic evaluation suggests that foreign policy can be every bit as much a matter of cultural creation as it is a function of social determination.

Inasmuch as this work was written by a diplomatic historian, one wishes that Cohen had spent a bit more time drawing out the implications of the relationships between art, culture, and power. But this is a quite minor complaint about a fresh and masterly work from a leading practitioner of his craft.

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DOUGLAS BRINKLEY. *Dean Acheson: The Cold War Years, 1953–71*. New Haven: Yale University Press. 1992. Pp. xiv, 429. $35.00.

Old diplomats never die—or so it sometimes seems to their successors. While in office, Dean Acheson only barely and not always kept to himself his dismal opinions of those who disagreed with him; once he left office his tongue loosened considerably. Acheson habitually blasted John Foster Dulles, his replacement as secretary of state, for deficiency in capacity, integrity, and vision. Having suffered the attacks of the radical Right during his own term in office, for the sake of what he deemed the greater good of American interests abroad, Acheson despised Dulles for appeasing the Rightists at what he accounted the expense of those interests; and his ill-will often showed.

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Although Acheson remained a loyal Democrat, his acerbic judgment did not spare fellow party members. He lashed Adlai Stevenson for indecisiveness, called John Kenneth Galbraith an ineffectual gadfly, and castigated John Kennedy for "treasonable" willingness to accept lax performance from his executive branch appointees (p. 137). George Kennan, Acheson's chief rival as expositor of the policy of containment, was dismissed as "a footnote of the Truman presidency" (p. 92).

Douglas Brinkley brings out the vituperative side of the older Acheson; he also reveals the steadfastness that lay beneath the quickness to criticize. In the late 1940s, Acheson had determined that the Soviet Union posed by far the greatest threat to global peace; despite the political changes that swept the face of the planet during the following two decades, he never altered this view. He was singularly unimpressed by the rise of Third World nationalism; he always believed that Europe was the centerpiece of international affairs. It was this belief that caused Acheson to decry the double cross—as he saw it—of Britain by Dulles and Dwight Eisenhower during the Suez crisis of 1956. Britain was worth a dozen Egypts in the struggle against the Soviets.

Brinkley's treatment of Acheson is fair and objective. He depicts Acheson slashing his opponents in the most arrogant manner; at the same time we see the elder statesman working assiduously behind the scenes to rescue the Democratic Party from what he considered a woolly minded liberalism in foreign affairs. Occasionally, Brinkley might have revealed more of Acheson's activities as presidential troubleshooter: he neglects, for instance, to note Acheson's advice to Lyndon Johnson in 1964 to let Turkey partition Cyprus and thereby lance that boil on the NATO body diplomatic. (Johnson rebuffed the suggestion, reasoning that one war—in Vietnam—was plenty in an election year.) Brinkley could have speculated further on the psychological origins of Acheson's overreactions to relatively minor policy differences. His responses suggest that he felt personally threatened by such differences. What was going on here? If Brinkley does not desire to put Acheson on the couch, figuratively speaking, he might at least sit him down in an easy chair, ply him with the cocktails he enjoyed, and try to get behind the bitterness that caused him to age so ungraciously.

Gracious or not, the post–State Department Acheson was a man worth listening to, and Brinkley has done students of recent American history a signal service in giving him his say.

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Elizabeth Anne Cobbs. *The Rich Neighbor Policy: Rockefeller and Kaiser in Brazil.* New Haven: Yale University Press. 1992. Pp. xiv, 273. $30.00.

A self-identified "bearer of good news" (p. 18), Elizabeth Anne Cobbs has written a laudatory case study of the diplomatic, philanthropic, and business activities in Brazil of Nelson Rockefeller and industrialist Henry J. Kaiser. In the period from 1945 to 1960, the United States focused its foreign aid programs on Europe and Asia and funneled few resources to Latin America. Alarmed that the United States was jeopardizing the Good Neighbor Policy and the special U.S.-Brazilian relationship, businessmen took the lead in promoting economic development through the transfer of U.S. management practices, technology, and capital. Determined to prove that private businesses could reap healthy profits while simultaneously fostering socially useful economic growth, Rockefeller financed rural development schemes, a mutual stock fund, and the International Basic Economy Corporation (IBEC), a capitalistic enterprise that aimed to raise Brazil's agricultural productivity. In Cobbs's view, Kaiser similarly practiced enlightened capitalism, establishing Brazil's first national automotive company, Willys Overland, and insisting that Brazilians control at least 51 percent of the company's stock and receive the technical training to take full control of the company. To be sure, not everything worked as planned; several of Rockefeller's enterprises failed, and Ford Motor eventually bought out the Brazilian owners of Willys. Yet this foreign investment had "useful outcomes" (p. 5). An IBEC subsidiary developed remarkable hybrid seeds that enhanced corn production, a staple crop in Brazil.

Beyond recounting Rockefeller and Kaiser's activities, Cobbs addresses vital historiographic questions. Her premise that analyses of foreign relations must include the role and impact of private individuals and organizations is a useful corrective to diplomatic histories that too often rely on official communications between governments. And her extensive research in both Brazilian and U.S. archives reminds historians of inter-American relations that the Latin American perspective is as important as the view from Washington. Cobbs also examines the increasing attraction among historians of twentieth-century U.S. foreign relations of the interpretive model "corporatism." As explained by Michael J. Hogan, Thomas J. McCormick, and others, the United States has tried to export the corporate ideal of government acting as a mediating force to rationalize business, promote self-regulation, stimulate economic growth, discourage redistribution, and coopt radical movements. But for Latin America, corporatism "is too coherent to describe U.S. policy" (p. 236). Although businessmen like Rockefeller favored corporatist methodology and ideology, the United States until 1960 opposed government-manipulated economic growth in Latin America.

With her celebration of Rockefeller and Kaiser's roles in Brazil, Cobbs has effectively challenged adherents of the dependency school who claim that U.S. foreign investment always distorts economic develop-

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ment in Latin America. Scholars will question, however, whether this revisionist case study is a unique story. Whether the efforts of Rockefeller and Kaiser were characteristic of U.S. private enterprise in Latin America remains an unresolved issue in Cobbs's work.

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CANADA

OLIVE PATRICIA DICKASON. *Canada's First Nations: A History of Founding Peoples from Earliest Times.* (The Civilization of the American Indian Series, number 208.) Norman: University of Oklahoma Press. 1992. Pp. 590. Cloth $39.95, paper $24.95.

This volume is a product of Canadians' growing interest in the history of native peoples of North America and their relations with Europeans over the last five hundred years. Olive Patricia Dickason focuses her considerable knowledge on these themes to produce a substantial work of many virtues.

Dickason makes a major contribution by sifting and weighing a vast body of existing research, including her own considerable work on France's Atlantic and St. Lawrence colonies. The 420 pages of text are accompanied by another hundred of notes, many of them miniature essays on particular points. The highly useful bibliography consists of three pages of primary sources and fifty-six of secondary works. All in all, this is a solid work that will be invaluable for both teachers and students of the history of Canada's original peoples.

Dickason utilizes two approaches. When dealing with the history of aboriginal peoples in the east, particularly prior to the nineteenth century, she examines the origins and history of each culture area. When her attention shifts to western and northern peoples, however, especially after the War of 1812, she employs a combination of geographical, cultural, and thematic treatments. Her examination of all regions and peoples is commendable for its thoroughness and insight, but the first two sections, on eastern nations down to the nineteenth century, are the most comprehensive, clearly argued, and valuable.

The study's weaknesses are three. First, Dickason's approach causes organizational problems for the reader. In particular, beginning students might find the material on the nineteenth and twentieth centuries oddly arranged and somewhat confusing. For example, chapter 17, "Pre-Confederation Administration in the Canadas," opens in Upper Canada, shifts north to the Arctic and subarctic, dodges southeast to the Maritimes, subsequently returns to Upper Canada, and in its closing pages takes the reader to the prairies and British Columbia. The last two substantive chapters similarly drop the reader into a jumble, the focus moving back and forth chronologically and skipping about thematically. In the space of a few pages (pp. 409–13), for example, one is introduced to issues of local political control, prominent Native Americans elected or appointed to office, a Supreme Court decision on the federal government's obligations as trustee, and Quebec's Cree-Naskapi Act. No survey can solve all the organizational problems its subject raises, but this one seems less successful than it might have been.

A second weak spot is Dickason's treatment of Christian missionaries and their participation in assimilative programs after 1830. She demonstrates little understanding of the evangelical mind-set. Specific topics such as residential schools and the nineteenth-century history of the mission settlement of Oka, where violence erupted in the summer of 1990, are scanned rather than probed in depth. Dickason's treatment of late nineteenth-century Oka inexplicably relies heavily on the account of a Protestant missionary, Amand Parent, who was a participant in some of the disputes at the mission.

Finally, Dickason's grasp of aboriginal legal issues is less than sure. In her coverage of the landmark St. Catherines Milling decision of 1889, the case's critical definition of aboriginal title is portrayed as the opinion of "one of the [law] lords" (p. 342), when it was a view shared by the Judicial Committee of the Privy Council. Her examination of more recent decisions by Canadian courts is also flawed. The negative rulings in both the Bear Island case in the Temagami district of Ontario and the McEachern ruling on the Gitksan case in British Columbia are treated at great length, although neither was a definitive ruling, emanating as they did from provincial appeals courts rather than the Supreme Court of Canada. Positive rulings from the highest court, such as in the Musqueam, Sparrow, and Sioui cases, are given less emphasis than negative decisions from lower courts.

Any work as ambitious as this one is bound to have some weaknesses. The ones mentioned here by no means render Dickason's treatment of a vast and complex subject unimportant.

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STEPHEN J. HORNSBY. *Nineteenth-Century Cape Breton: A Historical Geography.* Buffalo, N.Y.: McGill-Queen's University Press. 1992. Pp. xxvi, 274. $44.95.

Cape Breton Island, an area of 10,311 square kilometers, is one of a number of large islands off the eastern and western coasts of Canada that are now or once were separate political jurisdictions. Known to the French as Isle Royale, it was in the early eighteenth century the home of Louisbourg, that great garrison town captured twice (1745, 1758) by the British. It was transferred to the British in the Treaty of Paris in 1763 and was made part of Nova Scotia. It

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became a separate colony in 1784 but never achieved representative government, and it was in 1820 reannexed to Nova Scotia. Cape Breton became a loyalist refuge in the 1780s and then served as a major destination for Scots immigrating to British North America. Apart from its rich coal reserves, Cape Breton's major claim to fame has always been as the center of Gaelic language and culture in North America. In this excellent study of nineteenth-century Cape Breton, geographer Stephen J. Hornsby explores a number of major themes in economic and social development, particularly natural resources (especially coal), Scottish immigration, and Gaelic cultural retention.

Thousands of Gaelic-speaking Highland Scots came to Cape Breton in the first half of the nineteenth century. The earliest arrivals, who both had the choice of the best land and arrived with some financial resources at their disposal, did reasonably well. Later immigrants did not. The crofters and cottars who crowded onto the marginal backcountry of the island after the 1820s could not afford to buy land, and at the same time government policy greatly increased its price. The land, moreover, was not very fertile. The result was much squatting and an inexorable movement toward occupational pluralism. Ironically, even before the torrent of immigration to Cape Breton had ended, the island was experiencing famine conditions not unlike those driving people from Great Britain. The island began producing its own emigration to Newfoundland and New Zealand. The success of Gaelic culture came in the context of the increasing isolation and poverty of rural areas on Cape Breton. As in the highlands of Scotland, Gaelic language and culture flourished in a direct relationship with economic marginality. The retention of Gaelic increasingly marked a society that had lost its energetic and ambitious members to outward migration.

Like other jurisdictions in the Atlantic region, Cape Breton Island never benefited much from its considerable natural resources. Hornsby joins the debate over the underdevelopment of the Atlantic economy, emphasizing the island's fragmentation. On Cape Breton, he insists, staples enclaves were scattered and externally controlled. Industrialization was as fragmented as the resource economy that had preceded it, and a take-off point was never reached. Hornsby leaves the island at the end of the nineteenth century at the crossroads. It was thickly settled and ready to be part of an industrial renaissance of the Maritimes that would never really happen.

Like many geographers, Hornsby appears uncomfortable with the political dimension and almost totally ignores it in this work. Although he does suggest some comparative sense, particularly in relationship to other islands similar to Cape Breton in the nineteenth century, a greater focus on the patterns characteristic of relatively large (and geographically isolated) islands would have proved both useful and informative. At the same time, readers will learn much from this useful and informative study about Cape Breton's nineteenth-century development. There are a number of interesting maps and figures.

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David B. Marshall. *Secularizing the Faith: Canadian Protestant Clergy and the Crisis of Belief, 1850–1940.* Buffalo, N.Y.: University of Toronto Press. 1992. Pp. viii, 325. Cloth $55.00, paper $19.95.

The recent revival of religion as a topic of study in Canadian historical scholarship has been marked by two primary concerns: to demonstrate the important role religion has played in Canadian society and culture and to search for ways of analyzing and explaining this significance. David B. Marshall's book makes an important contribution to this emerging scholarly discourse. Indeed, Marshall is able to shift the focus of analysis to a question that in many respects develops logically from the scholarship published to date: given the almost unassailable position evangelical Protestantism enjoyed in the nineteenth century, how can one explain its undoubted decline in the twentieth? Given the ascendancy of religious ways of thinking, how can one account for the triumph of the forces of secularization?

Marshall answers this question with extraordinary force and clarity. In his view the Protestant churches had two options when they confronted the secular forces that were shaping the modern world: they could try to accommodate Christianity to the new order by bringing their message in line with the social, intellectual, and cultural currents of the present day; or they could ignore contemporary trends, secure themselves within the doctrines of the faith, and continue to preach the unalloyed truths of evangelical Christianity. The churches chose the path of accommodation, a strategy that not only failed to achieve its goal, Marshall argues, but also hastened the fate the churches feared so mightily. They hoped a more relevant message would spur on the building of the Kingdom of God on earth; instead it increased the pace of secularization and reduced the churches to a marginal force in contemporary society. If Christianity stood at the crossroads, this book is a lament for the road not taken.

Marshall documents this sad journey with an impressive array of characters, movements, and events. We follow the intellectual history of the Presbyterian, Methodist, and Baptist churches as they move slowly toward a liberal theology; we watch this theology struggling with the horrors of World War I and the ravages of the Great Depression; we see the challenges presented by a number of religious movements, from traditional fundamentalists to the well-heeled revivalists of the Oxford Group; and along the

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way we are introduced to important religious figures such as Charles W. Gordon, George Campbell Pidgeon, and Edmund H. Oliver.

The story throughout remains almost painfully constant. Although the official position of the Protestant churches was liberal and optimistic, many of their leaders expressed serious doubts about the present state and future prospects of organized religion. Their pessimism, Marshall maintains, speaks to the fact that, in reconciling themselves to the world, the churches lost far more than they gained and in the end religion was displaced by a culture that was thoroughly secularist in its ideas and outlook. The faith was secularized not by the powerful social forces of consumerism and capitalism but by a failure of the faithful; not by the quest for secular profits but by the secular quest of the prophets themselves.

Marshall's argument offers a necessary counterpoint to the unbridled religious optimism espoused by many Victorians, an optimism that has been echoed perhaps too loudly by a number of Canadian religious historians, including Richard Allen, John Webster Grant, Michael Gauvreau, and myself. Marshall brings us back to earth. The book's excellent account of the spiritual problems posed by global war and economic depression also raises a new set of issues that will excite Canadian scholars for some time to come. If, as Marshall suggests, the clash between postmillennial optimism and the cold, disarming reality of hunger, violence, and death marked the shortcomings of liberal optimism, it also provided the crucible for a whole host of religious movements, from fundamentalism to pacifism to the first cries of post-Christian existentialism.

A number of important questions, however, remain, especially about the question that is at the heart of the book: what the relationship is between religious change and the process of secularization. In addressing this issue Marshall accepts too readily the stance taken by his main characters, who tended to define the problem as a confrontation between organized religion and the forces of secularization. As an analytical framework, however, such a radical distinction between religion and society, between a well-established set of evangelical doctrines and the forces of the modern world, tends to put asunder what history (perhaps regrettably) has joined together. For example, I doubt if one can so easily separate religious doctrines from society and then argue that the quality of these doctrines in some way determined the pace and impact of social change (in this case that a failure to maintain doctrinal purity led to secularization). If the churches had not flirted with liberalism and had adopted the fundamentalist alternative, would anything have been different? Perhaps idealism should move its throne a little closer to the incoming tide.

This book's framework also distorts the character of evangelicalism itself. Although contemporaries may have regarded evangelical doctrine as a pure gift from a timeless and transcendent God, a wider historical perspective suggests that such orthodoxy was a relatively recent creation, the outcome of an accommodation in the early nineteenth century between older religious beliefs and what was then the modern world. One indeed might even be so bold as to suggest that evangelicalism—with its powerful critique of traditional religious and social hierarchies, its emphasis on practical results rather than doctrinal purity, and its abiding concern with individual salvation—was one of the cornerstones of the modern world itself. Evangelicalism, in effect, was part of the process of secularization that religion was so determined to oppose.

The relationship between religion and society is extraordinarily complex. Marguerite Van Die has argued that there is an ongoing tension within Christianity between Christ and culture, and that the history of religion has been marked by a constant process of redefinition and adjustment. Society has become embedded within religion as religion is embedded within society. Marshall presents a fascinating account of evangelical Protestantism's encounter with the world, an encounter in which the relationship between religion and society changed very dramatically, but I am not sure that for all its force and clarity his singular thesis can bear the full weight of explaining such a complex historical process.

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BILL RAWLING. *Surviving Trench Warfare: Technology and the Canadian Corps, 1914–1918*. Buffalo, N.Y.: University of Toronto Press. 1992. Pp. 325. Cloth $55.00, paper $19.95.

Several years ago naval historian Marc Milner lamented that there were serious gaps in the writing of Canadian military history ("Reflections on the State of the Canadian Army History in the Two World Wars," *Acadiensis* [Spring 1989], 139–41). Few historians seemed willing to tackle investigations of how Canada's soldiers learned their skills and managed their formations in battle. Since that time J. A. English has helped fill the gap for World War II with his perceptive and sometimes unflattering assessment of the Canadians in Normandy (*The Canadian Army and the Normandy Campaign: A Study of Failure in High Command* [1991]). Now Bill Rawling has done a similar valuable service for World War I.

This book is about the horror of industrial war. The men of the Canadian Corps learned how to use the machines of war to break the fixed linear front, but it was a bloody and slow business. Until after the battles of the Somme in 1916, the unholy "defensive trinity" of the spade, the bullet, and barbed wire, as J. F. C. Fuller allegorically put it (*The Conduct of War, 1789–1961* [1961], 160), caused Canadians to trade for little captured territory far more lives than it was

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worth. They had not yet developed the technical skills to use, modify, or invent weapons and tactics to overcome the Moloch of fire industrial war had given the other side. Canadians had no influence or control over the policy that kept them in the war. All a commander could do was to figure out how to survive; the tough truth was that high casualties forced innovation.

Rawling shows how, after the Somme, tactics evolved to provide alternative ways to fight battles. By 1918 Canadians were capturing their objectives with "clockwork regularity" (p. 220). In the last three months of war, the so-called "hundred days," the corps used tactics and tools to produce fluid battles that broke the grinding stalemate of the previous four years. Small units moved forward under creeping artillery barrage, often supported by tanks and using their own weapons. The Lewis gun, the Stokes mortar, the Mills bomb, the rifle, and the rifle grenade had all become the tools of troops who were both specialists in specific tactical roles and jacks-of-all-trades, able to use anyone else's weapon if necessary. In short, they had discovered how to manage and use the technology of industrial warfare that made the lives they traded for the enemy's ground worth it. The disturbing part of Rawling's message is that the evolving technology of war could not—and would not in any future war—significantly reduce the casualties, "it could only change the ratio between losses and gains" (p. 215).

To write this book Rawling has mustered an impressive amount of technical and tactical detail, mostly from primary sources. He has woven this detail into a smooth and convincing argument. The book has instructive appendixes on the corps' casualties and its organization. In the latter case, even a brief glance at the changes in its order of battle between 1915 and 1918 shows the reader how simple citizen-soldiers had been transformed into an army of battle-winning technicians. There are also useful diagrams and photographs as well as a good bibliographical essay.

Occasionally, some things can aggravate, such as typos in the text and the lack of maps. Sometimes Rawling jumps on the bandwagon of earlier historical interpreters. For instance, we hear that Sam Hughes, the Canadian Militia Minister, "was not interested in professionals. He believed fervently that all Canada needed were citizen-soldiers who could ride and shoot" (p. 13), none of which recognizes that Hughes was more fervent about innovation, training, and good equipment than Rawling's descriptions admit. Although Rawling may be explaining how Canadians learned lessons on the field, he does not show that they, like all generations in war, came to the field with preconceptions that had to be broken down by fire. In democratic societies it is the necessary penalty of peacetime. The poor Ross rifle also suffers. In describing the adoption of this Canadian weapon in 1901, Rawling tells us that the rifle was heavy and "abnormally" long, "a serious encumbrance in the trenches" (p. 11). The author does not point out that there were three marks of the Ross. The one he describes as the first version was in fact the mark III (model 1910), of which limited production started in 1912; most earlier marks were much shorter and lighter. Moreover, the mark III was no heavier or longer than the standard infantry rifle of any of the major powers in the war save England. All these nations had to learn the same lesson, that long rifles do not handle as well in trenches. But then again, who thought there would be trenches or a fixed front in 1901, or 1912 for that matter?

In spite of these minor aggravations, Rawling has written a good and useful book. It addresses an obvious omission in the type of military history available to Canadians. It points out not the glory of war but the agony and the marvel that Canadian soldiers could adapt to and master the challenges of industrial war.

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LATIN AMERICA

RICHARD L. HARRIS. *Marxism, Socialism, and Democracy in Latin America*. (Latin American Perspectives Series, number 8.) Boulder, Colo.: Westview. 1992. Pp. xiii, 234. Cloth $44.00, paper $16.95.

This book rests on the premise that Marxism retains great relevance for those interested in fundamental social change in Latin America and the Caribbean. Richard L. Harris attempts to demonstrate that relevance by examining the attempts at socialist transformation within the area. He does this by looking at the various categories of problems facing any state engaged in socialist construction. In each area he discusses how early theoreticians (Karl Marx and Friedrich Engels, Rosa Luxemburg, V. I. Lenin, and so on) viewed the problem, how subsequent practitioners (Mao Zedong) modified their thought, and then at how modern critics (Branco Horvat) have viewed the same problems. He then examines the way Marxist governments have operated in specific situations, including material on Russia, China, and Vietnam, among others. Finally, he shows how Latin American and Caribbean cases have differed or not, citing Chile, Cuba, El Salvador, Grenada, and Nicaragua, with occasional references to Bolivia or Brazil.

In separate chapters Harris treats the questions of democracy and dictatorship, stages of the transition, the expropriation of capital, the socialization of agriculture, the development of the forces of production, moral versus material incentives, socialist forms of organization, ideology and culture, and the emancipation of women. Harris argues that no correct or exact way exists to successfully handle each problem. Every situation must be treated separately depending on existing external and internal factors. Although

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guiding principles such as the development of popular and democratic institutions and workers' power exist, each government must work out the exact way to foster them.

Harris argues that in non–Latin American cases, socialist principles were either abandoned or deformed, leading to the growth of new elites, bureaucratic states, and/or a lack of true socialist participation. Real worker control, for example, was never implemented anywhere. Similarly, in the Latin American and Caribbean cases, revolutionary regimes and movements have either not gone far enough in encouraging transitional institutions and practices or else never even tried to implement a truly socialist program, as in Grenada or Nicaragua. Harris thus distinguishes between governments committed to a transition to socialism and ones dedicated to a nationalist, anti-imperialist platform.

Harris recognizes that given the present neo-liberal hegemony and the dominant position of the United States in the world, socialist revolution is not on the immediate agenda. Socialists must therefore build the basis of democratic organizations at the grass-roots level and at the point of production. They must organize among the "lower and middle classes" and form coalitions and popular front movements. Armed struggle is not now a viable option, although it should never be discarded entirely. At this point, electoral means and popular protest or insurrection seem the most fruitful avenues.

Harris has clearly done his homework in Marxist theory and practice. He also evokes his own personal experiences. Many will object to his basic assumptions. Others will disagree with his mostly negative assessments of every transitional situation and a holier-than-thou tone that occasionally creeps into the work. Nevertheless, this is a book that anyone interested in alternatives to dependent capitalist development should read and think about. Many of the themes and topics raised remain important not only because they grapple with the issue of development with social justice but also because they inform an important part of the current debate inside of Latin America among both intellectuals and political activists.

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Noble David Cook and W. George Lovell, editors. *"Secret Judgments of God": Old World Disease in Colonial Spanish America.* (The Civilization of the American Indian Series, number 205.) Norman: University of Oklahoma Press. 1991. Pp. xxiii, 285. $28.95.

This book is a well-edited collection of eight essays on the catastrophic impact of Old World diseases on Indian populations in colonial Spanish America. Perhaps 90 percent of native peoples were destroyed within the first century after European contact, the greatest loss of human life in history. Before the discovery of the germ theory it seemed that these inexplicable plagues must have been a "secret judgment of God."

No single author or discipline could have produced a work of such depth and diversity. The authors include historians, geographers, and anthropologists. Regionally, five of their essays deal with Andean countries (Chile, Bolivia, Colombia, and two on Ecuador), and there is one each on Guatemala and Mexico. The authors have done much archival research, using *cabildo* and court records, baptismal and burial rolls, tribute lists, traveler's accounts, and so on. To their credit, they have discovered much new information, but even so, with rare exceptions, the surviving documentary record is so sparse that conclusions, especially for the sixteenth century, are often speculative.

Woodrow Borah's introduction provides a general classification of epidemic studies and an overview of sources, approaches, and methodologies. Insightful suggestions are offered for future research strategies. Hanns J. Prem's essay on "Disease Outbreaks in Central Mexico during the Sixteenth Century" describes and tentatively identifies epidemics of smallpox, typhus, and measles. Prem argues for an abrupt population decline in Central Mexico starting with the great smallpox epidemic of 1520–21 rather than "the gradually increasing course assumed earlier by [Noble David] Cook and Borah" (p. 48).

W. George Lovell, in "Disease and Depopulation in Early Colonial Guatemala," suggests that the central question in Latin American historical demography is no longer how many Indians there once were, or might have been, but what caused so many to perish so quickly (p. 49). Thus, he downgrades the sometimes acrimonious scholarly disputes over "high count" and "low count." Furthermore, he states that most scholars now accept that it was disease, not "Spanish action"—Bartolomé de Las Casas notwithstanding—that was the critical factor in Indian depopulation, not that other nonbiological variables should be ignored.

Linda A. Newsom's "Old World Epidemics in Early Colonial Ecuador" identifies some nineteen epidemics between 1524 and 1618. Her analysis of fragmentary data is imaginative. She finds evidence of seven or eight possible Old World diseases present in Ecuador in those years, and suggests also that scholars should take more heed of the impact of epidemics on fertility rates.

In "Epidemic Disease in the Sabana de Bogotá, 1536–1810," Juan A. Villamarín and Judith E. Villamarín offer a useful disease chronology, chiefly of smallpox, measles, and typhus. Little social or economic context can be provided since the surviving documentation is so sparse. In "Death in Aymaya of Upper Peru, 1580–1623," Brian Evans offers a community-level examination of patterns of birth and

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death in one Andean village. He offers brief but suggestive associations between demographic trends and the local economy.

The two papers on the eighteenth century stand out for their depth of research and broader socioeconomic context, reflecting the relative abundance of documentary sources for that era. Suzanne Austin Alchon in "Disease, Population, and Public Health in Eighteenth-Century Quito" offers a solid contribution related to three issues: identification of epidemics, their demographic impact, and the response of local officials. Likewise Fernando Casanueva, in "Smallpox and War in Southern Chile in the Late Eighteenth Century," has excellent sources and effectively relates smallpox epidemics among the Mapuche and other Indian groups to Spanish regional and imperial policy.

One of the best features of this fine volume is the concluding chapter, "Unraveling the Web of Disease" by the two editors, Cook and Lovell. They offer a coherent discussion of the entire range of issues from earlier essays, including brief but useful summaries of the etiology, symptoms, and vectors of the various diseases. The volume features excellent maps and detailed charts, all of which are clearly integrated into the text. On the whole this is a judicious and balanced work, and a model of interdisciplinary cooperation. It represents a substantial advance in our knowledge of Spanish American colonial epidemics and their impact on Indian depopulation.

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JAMES E. MCCLELLAN III. *Colonialism and Science: Saint Domingue in the Old Regime*. Baltimore: Johns Hopkins University Press. 1992. Pp. xviii, 393. $52.00.

Eighteenth-century Saint Domingue (today Haiti) was the world's leading producer of sugar and coffee as well as France's most profitable colony. James E. McClellan III's study proposes to highlight the pivotal role of Saint Domingue in the world economy by focusing on the fascinating interactions of colonialism and science. Science formed part of and facilitated colonial development; simultaneously, the colonial experience affected science and scientific enterprise (science being, in the author's view, not a monolithic entity, but a dynamic complex of ideas, institutions, social groups, and individuals who embody and define natural knowledge).

Science was an inherent part of French colonialism and was institutionalized in the metropolis' culture and state apparatus. Although in Saint Domingue science served to develop colonial cartography and economic botany, it also supported slavery and the slave system (planters benefited from their slaves' inoculation against smallpox, for example). Moreover, it was promoted by the French mercantilistic state rather than by individual colonists and colonial enterprises. In other words, in the case of Saint Domingue science was not a force of progress but "an agent beholden to reactionary powers and the entrenched interests of the status quo" (p. 9). Not surprisingly, therefore, colonial science in Saint Domingue vanished in the wake of the French and Haitian revolution.

Although McClellan raises fundamental theoretical questions in his introduction, his study is mostly descriptive. The first part of the book—primarily based on a description of Saint Domingue published in the late 1790s—presents the material setting (geography, climate, diseases, flora, and fauna), the historical development of the colony, as well as eighteenth-century society and economy. A substantial section describes the cities and towns of the colony, where only 8 percent of the population lived but where science flourished.

The second part of the study analyzes science in colonial Saint Domingue, using a variety of archival and published sources. The author shows that science accompanied the French colonial enterprise from its beginning in the 1620s with the naturalist religious missionaries. In the early 1800s the French state promoted astronomical, cartographic, and botanical expeditions. Later, as the colony's importance to France increased, so did science in Saint Domingue. Medicine and medical infrastructure became a state priority in order to guarantee the welfare of colonists and their slaves, thus the prosperity of the colony. Following the model of the metropolis, a highly stratified medical infrastructure was established, supported by a similarly stratified medical profession. Racism and the fear of poisoning by slaves, however, regulated the practice of science, and legislation prohibited people of African descent, free or slave, from manipulating drugs and poisons. State-supported science was also directed toward applied botanical research and development in order to promote the production of useful plants. In particular, the Royal Jardin of Port-au-Prince played a leading role in the beginning of cochineal dye production in Saint Domingue. Meteorology encountered marginal success, but popular science, especially mesmerism or the fad for "animal magnetism," spread among the colony's urban population. Even some planters succumbed to the mesmeric craze and submitted their slaves to ethereal fluids in order to maximize their profits. Here again, racism ruled, and black and mulatto followers of mesmerism were severely punished.

Controversy over the scientific character of mesmerism triggered in 1784 the foundation of the Cercle des Philadelphes, the subject of the third part of the book. McClellan views the establishment in Saint Domingue of this scientific society by physicians and other men of the ruling elite and its rapid transformation into an official colonial institution as a landmark in the history of science and colonialism.

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He describes with minute detail the development of the Cercle from its creation to its recognition by Louis XVI in 1789 as well as its activities, its areas of research, its membership, and its disappearance in 1792 after the launching of the Haitian Revolution.

Elegantly written and illustrated, McClellan's study contains a wealth of information for readers interested in the history of Saint Domingue and science. Much of this information, however, is often overshadowed by details or interpretive debate that will interest only a handful of specialists. Moreover, more discussion is warranted of theoretical issues, namely science and the upholding of the status quo.

ALINE HELG
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MICHAEL CRATON and GAIL SAUNDERS. *Islanders in the Stream: A History of the Bahamian People*. Volume 1, *From Aboriginal Times to the End of Slavery*. Athens: University of Georgia Press. 1992. Pp. xxiii, 455. $60.00.

At the margins of both the Caribbean and the North American mainland the 700-island Bahamian archipelago lies—like the central figure in the powerful Winslow Homer painting that graces this book's dust jacket—athwart the Gulf Stream. Its historical experience (wrecking, piracy, salt-making, bootlegging, tourism, and offshore banking) has marked it off from the plantation islands to its south. And now, on the eve of its twentieth anniversary of independence from Great Britain, this young nation is the recipient of a comprehensive history written by two committed Caribbeanists: Michael Craton, author of several major studies of Caribbean slavery, and Gail Saunders, Bahamian historian and director of the country's department of archives.

The first of two chronological volumes, this book is framed without apology as a nation-building project, intended "to place the archipelago and its people more clearly in their regional contexts [and] to provide a much-needed sense of identity for the new nation" (pp. xii–xiii). The authors "believe that what Bahamians need is an authentic social history, one that will search, feel, and declare the true bedrock, roots, and groundswell of their special identity, and thus help to protect it for the future" (p. xvii). And they commit themselves to giving "recognition to ordinary Bahamians and to tell[ing] the story of their long struggle for self-realization against these same elites to whom history has hitherto belonged" (p. xx).

They begin their story with a four-chapter section devoted to the world of the Lucayan Arawaks (500–1525), but these "purest Bahamians," who are pictured as having inadequately defended their homeland, come across ultimately as characters in a morality play: "a copper-skinned people of handsome physique . . . [whose] tragic demise provided a model, and a moral, for all those who in the subsequent five hundred years have tried to make the Bahamas their home" (pp. 25, 58–59). These chapters range from careful reviews of the archaeological record to dramatic imaginings about what Lucayans thought in general (what the authors call their "*mentalité*"), and sometimes in particular: "Columbus decided to sail onward . . . leaving the stunned Lucayans to digest the significance of [the Spaniards'] coming . . . Almost certainly, the Lucayans' feelings were a mixture of relief, excitement, and trepidation; a sense, perhaps, that their world was changed forever" (p. 51). In my estimation, the gentle Lucayans have not yet found their historian.

With their depiction of the early European settlers who arrived via Bermuda and soon created a "picaresque surface of scandal and skulduggery . . . peripheral to Antilles and mainland alike . . . a people apart" (pp. 92, 63), the authors move onto firmer historical ground. Yet this second section, covering the years 1647 to 1783, resolutely continues their nation-building agenda. They declare the era of piracy a "formative period in the evolution of the Bahamian national character: one in which a tendency toward opportunistic self-reliance reached its most extreme, even a brutal, form but was at the same time lastingly imprinted" (p. 104). And they then present, in turn, the dramatic expulsion of the pirates, an analysis of the Bahamas at mid-century, and an account of the end of the Old Regime, tracing throughout the uneven development of local institutions, with special attention to racial policies.

The final and longest section of the volume is devoted to "Loyalist Slavery, 1783–1834." The influx following the American Revolution "trebled the colony's population [and] raised the proportion of slaves and other blacks from one-half to three-quarters of the whole" (p. 179), and the authors ably trot out the full cliometric toolbox to analyze the various consequences, with the help of tables, figures, and discursive prose. A descriptive chapter on the lifeways of the elite is complemented by more in-depth treatments of the varied experiences of the enslaved, set in a comparative Caribbean context. And a chapter based on the diary of an 1830s out-island planter leads into an extensive description and analysis of the varieties of slave resistance. The work ends, a bit abruptly, with allusions to the complex economic, social, and cultural relations, otherwise neglected in this volume, between the slaves (and ex-slaves) and the significant numbers of free(d) Africans imported as indentured laborers during the nineteenth century.

A fuller judgment of this enterprise must await the publication of volume two. Undoubtedly, it will need to be weighed in part against Howard Johnson's rather differently inflected *The Bahamas in Slavery and Freedom* (1991). But already, in ambition, scope, and generous intent, both this book and its companion

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volume stand out as a special kind of Caribbean history.

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FREDERICK M. NUNN. *The Time of the Generals: Latin American Professional Militarism in World Perspective.* Lincoln: University of Nebraska Press. 1992. Pp. xv, 349. $50.00.

This study by Frederick M. Nunn deals primarily with assorted perspectives of various Brazilian, Argentinian, Peruvian, and Chilean officers that are meant to reflect significant currents of thought between 1964 and 1989. Secondarily, the book compares these views with various opinions held by selected military leaders in other countries in Latin America and on all other continents. This constitutes, of course, an ambitious enterprise. Unfortunately, the introduction reflects a somewhat typically presumptuous kind of evaluation that plagues the rest of this volume: namely, that the military has been a powerful political force in modern times. Rather, Nunn should have noted as well that this institution exerted significant political control in Latin America since the wars for independence.

Furthermore, in his introductory comments, Nunn observes flatly that the officers who guided recent South American military governments were "professionals" and "militarists" (p. 2). The often controversial or somewhat foggy nature of these terms should have been addressed throughout his study with clarity and consistency, beyond a fleetingly instructive commentary that is included in the preface. In essence, then, Nunn fails to connect a sharply defined fundamental terminology to his subsequent discussions, loose as they might be.

Nunn searches for, in his rather desultory fashion, an explanation of the assimilation by the military of emerging post–1945 patterns of thought. Yet his study lacks firm chronological or topical cohesion. In addition, there are significant gaps of information. Among snippets of sometimes more trivial material, one searches in vain for even minimal enlightenment concerning the impact on Latin America's military of the Castro regime or of the experience of Argentina's Dirty War, just to mention a couple of examples. Controversies among military factions are slighted. Although Nunn's discussions of tensions between the military and civilian sectors are of some merit, he essentially ignores showdowns between competing military-civilian coalitions, as in Argentina during the 1960s and 1970s. Nunn seems to be at his best when he analyzes aspects of cultural malaise among the military, who, for example, had to contend with the forces of materialism, hedonism, and pronounced individualism.

In short, this study lacks depth and a firm structure, including penetrating analyses of domestic historical trends within distinctive societies. In addition, Nunn should have curbed a bewildering predilection for discussing excerpted material without taking note of an article's or book's fuller context. The quality of his study pales strikingly in comparison with such classic works as Alfred Vagt's *History of Militarism* (1959) or even Nunn's own *The Military in Chilean History* (1976).

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DAIN BORGES. *The Family in Bahia, Brazil, 1870–1945.* Stanford: Stanford University Press. 1992. Pp. vi, 422. $52.50.

In this well-written book, Dain Borges returns to many of the themes of Brazilian family life developed by Gilberto Freyre, especially in *Sobrados e mucambos* (1936), in which Freyre described the disintegration of the patriarchal rural family as Brazilian society and culture urbanized in the nineteenth century. Borges adopts virtually intact the process of change outlined by Freyre, but he looks at it through the lens of institutions that sought to reform the family and the essentially conservative family culture that resisted change.

Borges outlines three family types that shared a common family culture: country, urban rich, and urban middle-class families. He concludes that neither doctors nor the liberal changes in law that accompanied the empire nor the church managed to change family culture. Bahian families seemed to have followed their traditions, their "common sense" rather than the advice of outside institutions. What, then, caused families to change from a patriarchal to a nuclear style? Using a sample of families drawn from genealogies, Borges shows that as the agricultural economy declined, one integral part of family strategies (which had depended on the agricultural frontier) failed, and elite families fell back on their alternate urban, political strategy. Bahian families began to limit their fertility and to encourage migration from Bahia to the cities of Rio de Janeiro and São Paulo.

The assumptions that underlie this book require special scrutiny in light of the direction of research on the Brazilian family. In his introduction, Borges states that his "study argues that we need to see the many forms of the family in Brazil as they existed as recognizable options or alternatives" (p. 7). The inescapable conclusion to be drawn from his book, however, is that the family in Bahia is the family of the white upper class. Borges himself states that because so many Bahians did not marry, marriage and legitimate family life became a "badge of social honor" that divided "the families" and "the people" (p. 274). This equation of family with elite is surprising given that most family historians have rejected Freyre's patriarchal family as normative for Brazil. Even the

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most rudimentary demographic analysis reveals that the vast majority of Brazilians lived in nuclear or female-headed families and that slaves and the free poor did form their own families. In Borges's defense, he is not interested in family household structures but rather in the set of social relations that determined how family members behaved. At minimum, his title is misleading; his book traces not the family in Bahia, but the upper-class family.

Even if this is intended as a study of the upper-class family, the absence of any systematic analysis of the way that Brazil's hierarchical class and racial structure influenced it is perplexing. Borges suggests that his book "examines families and cultural change from the point of view of institutions and their often conflictive relations" (p. 8). Yet his analysis focuses exclusively on influences from outside (Europe) or from those of the same class (doctors, lawyers, clergy). Surely this study, set during the abolition of slavery, warrants a chapter on the institution of slavery. Borges stresses the resilience of family "common sense" in the face of medical expertise, church dogma, newly promulgated laws, and foreign influences, yet here too he might have evaluated the influences of slaves and peasants on elite family culture.

Borges reminds us that family life is full of contradictions and inconsistencies. He is to be commended for working toward a synthesis of Brazilian family history. That synthesis will be achieved when the social environment of Brazil is fully addressed.

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PETER BLANCHARD. *Slavery and Abolition in Early Republican Peru.* (Latin American Silhouettes.) Wilmington, Del.: Scholarly Resources. 1992. Pp. xx, 247. Cloth $40.00, paper $15.95.

African slavery in Peru was an important and enduring institution, with roots in the coastal economy and society dating from the first half of the sixteenth century. Its peaceful abolition in 1854–55 receives careful study in Peter Blanchard's most welcome book. When Peru gained independence in 1825, its slave population numbered only some 50,000, or 3.8 percent of the country's 1.3 million inhabitants (among whom figured 40,000 free blacks), and most Peruvians confidently predicted slavery's imminent demise. At that date, not only had an end to the African slave trade been decreed but also a law of the free womb was enacted, whereby all children born of slaves in Peru would be free although under the tutelage of their mothers' masters as so-called *libertos* until they attained their majority.

Although small, Peru's slave population was economically important both in the Lima area and in other agricultural zones along the coast. Slaves made up a significant portion of the assets of many coastal plantations while the wages of urban slaves represented an important source of income to their masters. Faced with an economic slump during the 1830s and 1840s, many slaveholders stubbornly clung to the familiar ways of a declining institution and successfully pressured weak national governments into measures designed to postpone the inevitable. An abortive attempt was made to import slaves from Colombia; even more sinister was the proviso that *libertos* serve their masters until age fifty and in the interval be subject to sale as if they were slaves.

In sketching this recalcitrance, Blanchard may overemphasize the weight of tradition and greed while overlooking generalized slaveholder impoverishment. As he himself concedes (p. 59, n. 1), "the composition of the slaveholding group and the number of slaveholders during these years have yet to be studied in any detail," and without this information, set within the context of Peruvian economic development, the dilatoriness of abolition cannot be fully explicated. Blanchard seems on firmer ground when asserting that slaveholders had their way in large measure because no countervailing trends or groups opposed them. The guano boom produced no middle class for whom the continued existence of slavery might have seemed a distasteful anomaly. Despite widespread abolitionist sentiment, no party appeared to champion the cause, and even the bureaucrats, judges, and newspapermen who opposed slavery confessed their qualms about the control and socialization of Peru's black population in its absence. During this interval, it was perhaps the slaves themselves who contributed most to the weakening of slavery through their various efforts to secure manumission and better treatment. The full range of sentiment and interest in the master-slave relationship, however, needs to be explored in greater subtlety and detail than Blanchard permits himself here, and I expect that the forthcoming work of Christine Hünefeldt for this period will go far to fill this gap.

The end of Peruvian slavery finally came about as part of General Ramón Castilla's bid to topple a rival from the presidency. Both men appealed to the remaining slaves, but Castilla's offer was more generous, imposing few restrictions on freedom other than the general obligation to work. Slaveholders were mollified with the promise of compensation, a process largely completed by 1860. Although Blanchard does not pursue postabolition developments in detail, he has given us a tale well told and a solid foundation for future research.

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JAMES D. RUDOLPH. *Peru: The Evolution of a Crisis.* Foreword by CYNTHIA MCCLINTOCK. (Politics in Latin America.) Westport, Conn.: Praeger or Hoover Institution Press, Stanford, Calif. 1992. Pp. xii, 166. Cloth $42.95, paper $12.95.

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JOHN CRABTREE. *Peru under García: An Opportunity Lost.* (Pitt Latin American Series.) Pittsburgh, Pa.: University of Pittsburgh Press. 1992. Pp. xv, 236. $49.95.

"What happened in Peru?" might well be the most poignant question Latin American scholars will ask themselves as they assess the political lessons of the last quarter of the twentieth century. In the 1960s, Peru's military government was the exception to the dominant pattern of repressive regimes that characterized the region. Paradoxically, the return to democratic electoral norms in 1980 coincided with a declining economy and escalating political violence that combined to make Peru's experience with democratic reform the hemisphere's worst-case scenario. Most recently, President Alberto Fujimori has again made Peru the exception, giving it the dubious distinction of being the first tentative democracy in South America to tilt back toward authoritarian rule.

Peru exemplifies all of the complexities of modernization and development: rapid urbanization, social and political mobilization, military rule and reform, "stagflation," and structural adjustment. Change has been deeply disruptive, and the living conditions of the vast majority of Peruvians are precarious. Frustration with the response of political leaders and impatience with democratic procedures are widespread. The state's legitimacy and supremacy is contested by armed insurgents and the outcome of the struggle has been far from certain. Fujimori's flirtation with authoritarian rule is closely watched, not only by Peruvianists but also by other Latin American leaders contending with economic decline and democratic constraints. The question is, what went wrong in Peru and could it have been avoided?

The two books under review help answer that question. They are somewhat different in scope and style, yet both are well worth reading. They often arrive at similar conclusions about the critical mistakes made by the two democratic regimes they examine in detail (Fernando Belaúnde's second administration from 1980 to 1985 and Alan García's first from 1985 to 1990). Taken together, these books provide considerable insight into the challenges posed to democratic leaders throughout the Americas.

James D. Rudolph's study should be read first. Rudolph paints a portrait of Peru with extensive geographic and historical background. Readers will find his characterization of the political system thorough and his discussion of the Belaúnde and García regimes insightful. His discussion of the hierarchical structure of Apra and the dominant charismatic role of Victor Raúl Haya de la Torre is not only useful background for the discussion of the García administration but it also resonates with patterns found in discussion of the structure of *Sendero Luminoso*, the Shining Path insurgency. Rudolph's analysis of *desgobierno* during the García regime is especially well done. He gives a concise account of García's failures of judgment. The link Rudolph makes between the changing role of the state and *desgobierno* represents a substantial contribution to the current literature on Peru.

John Crabtree has done a wonderful service with his study of Peru under García. As he notes, there was a great deal of scholarly attention to the self-proclaimed revolutionary military regime of 1968–80. Despite that interest, a close analysis of the recent attempts at democracy has been lacking. Although he deals with the inheritance of the Belaúnde regime, his focus is on García, and his subtitle, "an opportunity lost," is a good indication of his conclusion. Crabtree's book is a superb blend of economics and politics, a *tour de force* of multidisciplinary integration and balance. He makes prodigious use of economic data, tracing the political and social consequences of shifts in economic strategies. Crabtree emphasizes the limits and weakness of the Peruvian state in a superb synthetic introduction that deserves careful reading. The book is well organized and the theoretical issues are well defined.

Both books have solid treatments of *Sendero Luminoso* and the role of drug trafficking in supporting that movement and the Peruvian economy as a whole. Crabtree does an excellent job describing Peruvian political parties and their role in Peru's predicament. This is welcome. Most studies of Peruvian politics emphasize the role of the president at the expense of political parties. Given the strength of the authoritarian tradition, the adoption of democratic procedures and institutions requires the construction of ties between leaders and the newly mobilized populace. We know too little about the role of political parties in this process and this knowledge is crucial to understanding the challenges and the resources available to would-be democrats.

In assessing the recent past, Crabtree and Rudolph concur in their fundamental findings: the economic crisis of 1982 was beyond Peru's control; the crisis of 1987–89 was largely self-imposed. The García government, and Alan García in particular, made some very bad choices. The worst choice by far was the attempt to nationalize the banks in July 1987. For Rudolph and Crabtree politics is still a human enterprise; culture, history, and the international system set the stage, but in political life, both leaders and citizens play critical roles. Fixing Peru may be a superhuman task, but understanding what happened is a very human responsibility.

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Collected Essays

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed.

GENERAL

JOHN DUNN, editor. *Democracy: The Unfinished Journey, 508 BC to AD 1993*. New York: Oxford University Press. 1992. Pp. xii, 290.

SIMON HORNBLOWER, Creation and Development of Democratic Institutions in Ancient Greece. CYNTHIA FARRAR, Ancient Greek Political Theory as a Response to Democracy. G. E. R. LLOYD, Democracy, Philosophy, and Science in Ancient Greece. QUENTIN SKINNER, The Italian City-Republics. DAVID WOOTTON, The Levellers. GORDON S. WOOD, *Democracy and the American Revolution*. BIANCAMARIA FONTANA, Democracy and the French Revolution. CHARLES S. MAIER, Democracy since the French Revolution. NEIL HARDING, The Marxist-Leninist Detour. SUNIL KHILNANI, India's Democratic Career. SUSAN MENDUS, Losing the Faith: Feminism and Democracy. NEAL ASCHERSON, 1989 in Eastern Europe: Constitutional Representative Government as a "Return to Normality"? JOHN DUNN, Conclusion.

LOUIS PATSOURAS and JACK RAY THOMAS, editors. *Essays on Socialism*. San Francisco: Edwin Mellen. 1992. Pp. viii, 409.

LOYD D. EASTON, Economic Democracy in Ohio's Owenite and Fourierist Communities. LOUIS PATSOURAS, Jean Grave: French Anarcho-Communist. WILLIAM J. FISHMAN, Rudolf Rocker: Revolutionary Syndicalist. PAUL LEBLANC, Towards an Understanding of the Leninist Party. LOUIS PATSOURAS, Thorstein Veblen's Socialism. MORRIS SLAVIN, Trotsky's Fight against Fascism. LOUIS PATSOURAS, Léon Blum and French Socialism. JACK RAY THOMAS, Salvador Allende. MICHAEL RAY LA CHAT, Christianity and Socialism: A Troeltschean Perspective. GARY SCOTT SMITH, Walter Rauschenbusch and the Social Gospel. C. M. KEMPTON HEWITT, The Marxist Jesus of Nazareth. JACK RAY THOMAS, Chilean Socialist Party Problems during World War II. LOUIS PATSOURAS, Marx's View of Class Boundaries and Polarity as Contrasted with Those of Mills, Dahrendorf, Poulantzas, Wright, and Braverman. BORIS BLICK, French Liberals and Anarchism in the Late Nineteenth Century.

JOHN M. FOSSEY, editor. *Proceedings of the First International Congress on the Hellenistic Diaspora from Antiquity to Modern Times*. Volume 2, *From 1453 to Modern Times*. (McGill University Monographs in Classical Archaeology and History, number 10.) Amsterdam: J. C. Gieben. 1991. Pp. xv, 508.

JOHN M. FOSSEY, Introduction. JOZE PIRJEVEC, The Greek Colony in Trieste in the 18th–19th Centuries. OLE L. SMITH, Greek Scholars in Renaissance Italy: Some Aspects of Their Rôle in the Dissemination of Classical Literature. SYSSE ENGBERG, Greek Literacy during the Tourkokratia and Manólis Glynzoúnis' Edition of the "Prophetie," Venice 1595/96. MIHAIL CARATAŞU, Pages de la correspondance échangée par Dosithée de Jérusalem avec les marchands de la "compagnie Grecque" de Sibiu (documents inédits). JACQUES BOUCHARD, Le type de l'"immigrant fortuné" dans l'œuvre de Nicolas Mavrocordatos. LIA BRAD-CHISACOF, Remarques linguistiques en marge des documents des XVIII^{ème} et XIX^{ème} siècles émanant des principautés roumaines. CORNELIA PAPACOSTEA-DANIELOPOLU, La diaspora hellénique des principautés roumaines et l'éclosion de l'idéologie moderne (1774–1821). OLGA CICANCI, La spécificité de la diaspora grecque dans l'espace roumain à l'époque moderne. ROXÁNE D. ARGYRÓPOULOS, A 19th Century Greek Scholar in Bucharest: Mikhaíl Khristarís and His Library. PETER I. HIDAS, The Greeks of Hungary. ÖDÖN FÜVES, Characteristics of the Greeks in Hungary (1550–1850). MARTA BUR, The "Greek Company" in Hungary in the 17th–18th Centuries. ALEXANDER KITROEFF, The Transformation of Homeland-Diaspora Relations: The Greek Case in the 19th–20th Centuries. ELENA FRANGAKIS-SYRRET, The Western Anatolian Coast and the Aigaian Islands in the Late 19th Century and Early 20th Century: An Economic Survey. ÉMILE KOLODNY, Les Grecs dans une grande ville d'Allemagne Fédérale: Le cas de Stuttgart (Bade-Wurtemberg). KÄTHI DORFMÜLLER-KARPUSA, The Biculturality of Immigrant Greek Children in West Germany. HANS EIDENEIER, Von "Apanos" bis "Kranfiras": Ein Beitrag zur Deutsch-Griechischen Lautkontrastivistik. GREGORY T. PAPANIKOS, Greek Emigration to Canada: Economic or Political? WILLIAM SCHABAS, The Greek Diaspora and Canadian Law. EFIE GAVAKI, Greeks in Montréal, Ethnic Identity, Family Structures and Conflict: An Intra- and Inter-Generational Perspective. CATERINA PIZANIAS, After the Odyssey, Babylon: The Social Construction of Ethnicity in the Hellenic Diaspora. DAVID B. RICKS, English Influences on Intellectuals of the Diaspora: C. F. Cavafy and Alexander Pallis. SMARO KAMBOURELI, A Palindromic Journey: Eyeing the Immigrant Writer.

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JOHN M. FOSSEY, editor. *Proceedings of the First International Congress on the Hellenistic Diaspora from Antiquity to Modern Times*. Volume 1, *From Antiquity to 1453*. (McGill University Monographs in Classical Archaeology and History, number 10.) Amsterdam: J. C. Gieben. 1991. Pp. xvii, 294.

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication is solely at the editor's discretion. Letters may not exceed seven hundred words for reviews and one thousand words for articles. They should be submitted in duplicate, typed double-spaced with wide margins, and headed "To the Editor."

REVIEWS OF BOOKS

To the Editor:

In her discussion of Dennis R. Dean's detailed and scholarly biography of James Hutton, *James Hutton and the History of Geology* (*AHR*, 98 [June 1993]: 861), Rachel Laudan does not review the content of the book. Instead, she dismisses the importance of Hutton on the basis of a "revisionist" estimate provided, she says, in the writings of Martin Rudwick, Roy Porter, Mott Greene, and herself. She suggests that Hutton's geological writings on heat did not appeal to Wernerian geologists on the Continent because of Hutton's deism and makes the astonishing and completely false statement, "As chemical theory changed and as new field evidence was collected, Wernerians quickly changed their minds about heat while remaining committed to the Wernerian program for mapping the formations around the globe and remaining open-minded on uniformitarianism." Wernerians clung obstinately to their beliefs, in most cases until death, and few of them ever understood or accepted uniformitarianism.

Abraham Gottlob Werner held that all rocks, including crystalline rocks, had precipitated successively from an ocean, as deep as the highest mountains, extending over the whole earth. Werner did not recognize the existence of igneous rocks or that sedimentary strata had been elevated or disturbed.

In 1796, James Hutton demonstrated that sedimentary strata, laid down beneath the sea in a horizontal position, when found in hills or mountains in inclined or vertical positions, had been both uplifted and disturbed by the internal heat of the earth. Hutton recognized that trap rocks and granite were igneous rocks, which, in contact with sedimentary rocks, had altered the latter by heat. Sedimentary strata, in Hutton's opinion, represented the detritus of preexisting land carried down by running water into the sea. Successive sedimentary formations, therefore, represented a succession of vanished lands extending indefinitely into the past.

In Britain, Werner's students bitterly opposed the Huttonian theory of basalt and trap rocks and of igneous veins and dykes. Until the 1820s, they continued to oppose the igneous origin of granite.

Hutton's position in the history of geology derives from the fundamental geological questions with which he dealt, from his keenness of perception as a field observer, and from his capacity to reason from his observations. It does not depend on a "canonical" historiography, even though Sir Charles Lyell's brief history in the *Principles of Geology* (1830–33) is accurate, based as it is on careful reading of the geological works to which he refers. The same cannot be said for some recent revisionist authors.

Leonard G. Wilson
University of Minnesota, Twin Cities

Rachel Laudan replies:

Leonard Wilson chides me for dismissing Hutton rather than reviewing the content of Dennis Dean's *James Hutton and the History of Geology*. This incorrectly characterizes the review, which does describe Dean's book. But it is true that I devoted a good part of the review to the historiographic context. Dean, after all, aims to show—against "revisionist historians ... [who] 'correct' the late nineteenth-century apotheosis of Hutton in various respects"—that Hutton "is far more the precursor of our own outlook than any other theorist one can name—and therefore, as so many of his beneficiaries have proclaimed, legitimately the founder of modern geology" (pp. 263 and 269). Given the rampant precursor-itis that gives Dean's book its structure, my criticisms were couched far more gently than they probably should have been.

Wilson proceeds to summarize the canonical Lyellian historiography, which most recent historians of science have called in question. He portrays a debate over the nature of granite and trap rocks in which

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Wernerians "cling obstinately" to their beliefs and "bitterly oppose" Huttonian theory. Such a narrow depiction of a debate about a subset of geological formations, formations whose origins Hutton purportedly got "right," obscures the reasons why Wernerians resisted accepting the Huttonian theory. For also at stake were—at the least—opposing theories of the chemistry of consolidation, of the origin of the sedimentary rocks, and of the nature of heat, as well as differing epistemologies, cosmologies, and theologies. In all these areas, Hutton assumed, but rarely made explicit, positions that were original and intriguing, but very dubious, to those Wernerians who could figure out what they were.

Wilson further charges that I have made a "completely false statement" about the Wernerian positions on heat and uniformitarianism. On the matter of heat, Wernerians had been prepared to contemplate a hot ocean from the start. Alexander von Humboldt and Leopold von Buch, Werner's most distinguished followers, devoted much of their careers to investigating igneous and metaphoric action. On the matter of uniformitarianism, we come to one of the murkiest concepts in the history of geology. For a full discussion of what it might mean, readers could turn to Martin Rudwick's discussion "Uniformity and Progression," in D. H. D. Roller's *Perspectives in the History of Science and Technology* (Norman, Okla., 1971) or my own "The Role of Methodology in Lyell's Geology," *Studies in History and Philosophy of Science*, 13 (1982): 215–50. If, however, with Dean and Wilson, one takes the simplistic notion of uniformitarianism to mean the "present is key to the past" (Dean, 263) or "gradualistic naturalism" as opposed to "cosmological myth" (Dean, 265), Werner would have to count as a uniformitarian, because he and his followers used observations of the present to reconstruct the geological past and because he was no subscriber to cosmological myths.

RACHEL LAUDAN
University of Hawaii

TO THE EDITOR:

I appreciated the thoughtful tone of Susan Dwyer Amussen's review of my book *The Discovery of Childhood in Puritan England* (*AHR*, 98 [June 1993]: 871). But I was dismayed at the main burden of her rather strenuous criticism. She asserts that I make no difference between Puritans (before 1600) and Dissenters, when in fact I made that my second thesis! At several points, I invoked the change from Puritanism (a movement) to Dissent (sects) to explain, however conjecturally, the souring of attitudes toward children (10f, 13f, 39, 55, 94, 132, 165f, 172). This is in line with my claim that the social dynamics of these groups determined their attitudes, rather than, say, their theology. And this makes irrelevant the criticism that "Anglican"/Puritan was not a major theological divide. Thus the work of Collinson, Tyacke, and Lake, which is not concerned with social organization, is beside my point.

In retrospect, I fear that I was not as clear as I should have been in arguing an unfamiliar thesis in unfamiliar terms, and I regret the resulting confusion.

C. JOHN SOMMERVILLE
Institute for Advanced Study, Princeton

SUSAN DWYER AMUSSEN REPLIES:

I am sorry that my review of C. John Sommerville's book erroneously suggested that I did not realize that he understood the difference between a movement and a sect. His letter does not, however, change my central criticism: Sommerville's chief argument is that Puritans discover childhood and that Anglicans do not discover childhood until after the Restoration. Because Puritans were Anglicans before 1640, this is misleading at the very least. While I agree with Sommerville that there are particular attitudes toward society that can usefully be categorized as "puritan," such attitudes existed among many people who thought of themselves very clearly as "Anglican."

Describing religious attitudes in early seventeenth-century England is, of course, notoriously difficult. When labels are used without adequate conceptual rigor and clarity, as they are by Sommerville, the result, unfortunately, obscures as much as it clarifies in our understanding of the past.

SUSAN DWYER AMUSSEN
Union Institute Graduate School

TO THE EDITOR:

Paul C. Merkley begins his review of Ursula M. Niebuhr's *Remembering Reinhold Niebuhr: Letters of Reinhold and Ursula M. Niebuhr* (*AHR*, 98 [June 1993]: 968–69) by telling us, "There is a story that goes with this book." The story concerns the "open secret" that her book is primarily a response to my *Reinhold Niebuhr: A Biography* (1985). Merkley then takes me to task for assertions he claims I made in my review of the book in *Commonweal* (January 31, 1992): 34–36, and in other unspecified places "on record." I have great respect for Merkley's own work on Niebuhr—his 1975 volume *Reinhold Niebuhr: A Political Account* is one of the best books on him. But I must protest his account of my *Commonweal* review and his decision to tell open secrets.

The beauty of the "open secret" is that it allows the teller to divulge his inside dope without revealing his source, since the secret is really not a secret after all; it is available to anyone whose ear is to the proper grapevine. The teller graciously spares us the trouble of tracking down the gossip on our own. We are so glad to make it into the circle of knowers, so struck by the authority of the teller who knew the secret before

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we did, that we overlook troubling issues. In the case of Merkley's review, if his source for Ursula Niebuhr's motivations is Ursula Niebuhr herself, that should raise questions about the propriety of having him review her book. If his source is not Ursula Niebuhr, that should raise questions about the reliability of the story. In either case, he should tell us the source, so we can decide for ourselves. If there is a compelling reason to keep the source secret, he should say so and explain why disclosure of the secret is necessary at all. I think book review editors should tell reviewers to avoid open secrets and stick to publicly available texts and publicly acknowledged sources.

As it happens, I may myself be Merkley's source, since two years ago he and I had a brief exchange about Ursula Niebuhr's book. As it happens, his story about Ursula Niebuhr's motivation is largely true, although it needs some qualification. But that does not change anything regarding the principle at stake. If I am his source, he should indicate as much. I am glad to report that whoever his source is, historians are not dependent on Merkley's open secret if we wish to grasp Ursula Niebuhr's motivation in assembling her book. Her letters deposited in the Reinhold Niebuhr Papers at the Library of Congress, and open to the public, establish that in producing her book she was accomplishing a task long projected and long deferred—ever since June Bingham's *Courage to Change*, the first biography of Reinhold, appeared in 1961—the task of setting down a portrait of her husband composed by the person who knew him most intimately for the second half of his life (they met when he was thirty-eight). Her letters make plain that my biography, like Bingham's and at least two other books that touched centrally on her husband's life, those by Ronald Stone and Larry Rasmussen, struck her as woefully deficient. In part, such failure was inevitable, she believed, since only a person with intimate knowledge of Niebuhr could produce a fully accurate account. In part, the failure stemmed from what she labeled the excessively "psychological" cast of my own and June Bingham's biographies. The idea for her book did not pop into her head when she read mine. Reading it was the stimulus that galvanized her to complete a book—by now a collection rather than a biography—that she had long yearned to publish.

In that sense, she was consciously responding to my biography, but her letters also make clear that she was targeting some reviews of the book, not just the book itself. She lamented that some reviews spotlighted what she considered mistakes in my reading of her husband's character. She thought those reviews made him appear cold, dour, unemotional, incapable of friendship, an undevoted father—descriptions that I believe no careful reader of my book would make. The remarkable thing to me about Ursula Niebuhr's book (as I wrote in *Commonweal*) is that it supplies much evidence, despite its intention, for the kind of "psychological" observation I made in my book. By including letters that are eloquent about some of Reinhold Niebuhr's doubts and insecurities, her book contributes to our understanding of the psychic drama that accompanied his public career, a psychic drama about which he himself was often very forthright.

In his report of my *Commonweal* review, Merkley writes that I "complained on record" there and elsewhere about Ursula Niebuhr's "unfair practice" toward Niebuhr scholars, and he implies that I failed "to concede a debt of gratitude" for her labor in putting together her book. He further alleges that I accused her (although he hedges on whether I did this in *Commonweal* or in some other unspecified place) of earlier denying "the very existence" of the letters she has now published in this book. These charges are egregious misrepresentations, as anyone who reads my review will readily discover. (I would be glad to send a copy to anyone who is interested in this dispute and does not have easy access to *Commonweal*.) In fact, I praised many features of her work and made no complaint whatsoever about unfairness on her part toward Niebuhr scholars. If Merkley believes I made this or any other charge in some other place, I wish he would specify which publication he is talking about so that I can respond.

Merkley chides "scholars" for expecting that "all the questions about Reinhold Niebuhr's character" could possibly be settled by the kind of evidence a widow's book would contain. "What serious scholar would go to a book like this with such an expectation?" he asks accusingly. But I never expressed such an expectation. I agree with Merkley that no widow or widower is obliged to "expose to the world" the whole story of their private relationship. I did note, as Merkley does, that the letters she offers in the book have been edited, and others, assuming they exist, have been left out altogether. And I said that the omission of a letter, or at least a retrospective editorial comment, is especially unfortunate when its inclusion would answer a crucial question posed by another letter already included in the book—such as the one from Reinhold in which he asks her tentatively about her own desire for "vocational work." Of course, it would be nice for us scholars if she had felt able to include full texts of all letters, but I have no complaint whatsoever about her decision. Scholars can hope that a person's sense of privacy will not limit their access to important documents, but that hope does not entail disrespect for the person's choice, whatever it may be.

Merkley does say some nice things about my work on Niebuhr, for which I am grateful, and I repeat that his own book is well worth consulting. It is a lot better than his review, where he uses so much of his space to discuss my work that he seriously slights Ursula Niebuhr's. One would never know from his review that the volume is more than a collection of letters. It contains some excellent writing of her own. Her recollections as well as her letters reveal a gifted

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writer at work, as I stressed in *Commonweal*. It is no accident that such lovers of language as W. H. Auden and Isaiah Berlin maintained long correspondences with her, in which she wrote lively epistles of whimsical, stream-of-consciousness prose.

The letters published in *Remembering Reinhold Niebuhr* will reopen the quest to link his ideas and his life. It is to Ursula Niebuhr's great credit that she has given us so much compelling evidence for that effort, even when the evidence works against her own conclusions about the "real" Reinhold. By publishing these letters, she takes us more fully into Reinhold's character than I was able to go a decade ago. "I am really so much married," Reinhold wrote to her in 1936, "that I feel as if I had lost my personal identity." Ursula shared with Reinhold not just a relationship but an identity. "There is no question," I concluded in *Commonweal*, "that she has given us something in this volume that no one else could provide: an account of that part of him, inseparable from the rest, that was Ursula herself."

RICHARD WIGHTMAN FOX
Boston University

Paul C. Merkley does not wish to respond.

THE EDITOR

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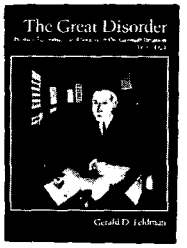
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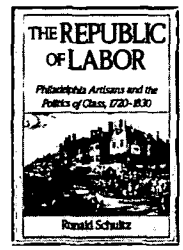
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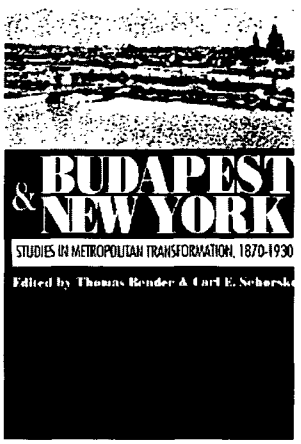
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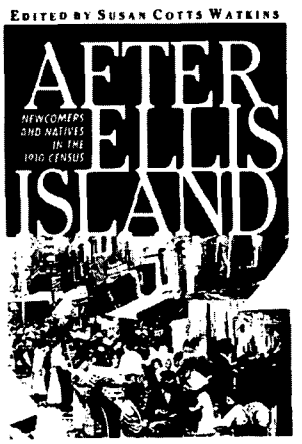
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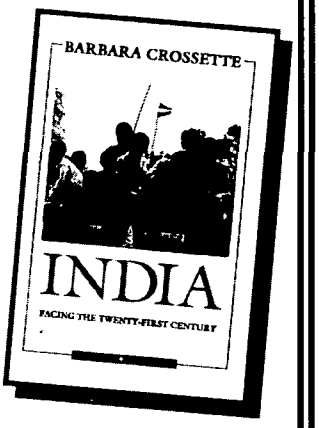
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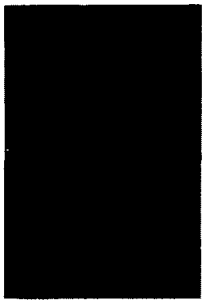
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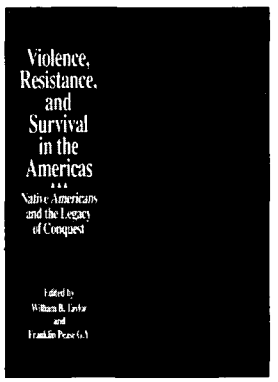
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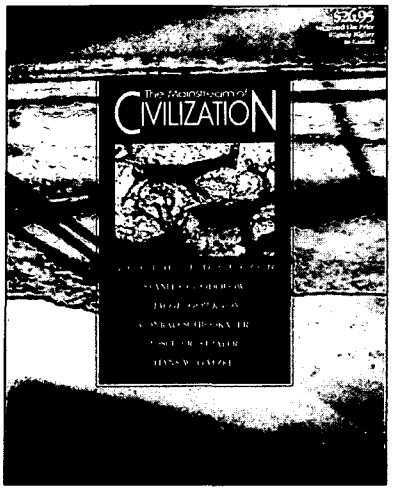
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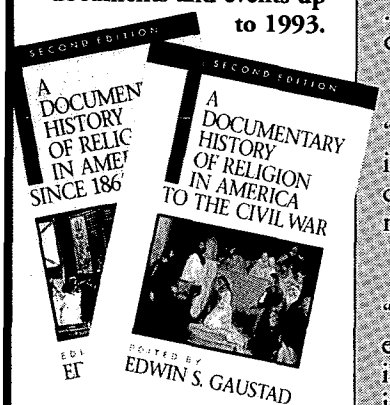
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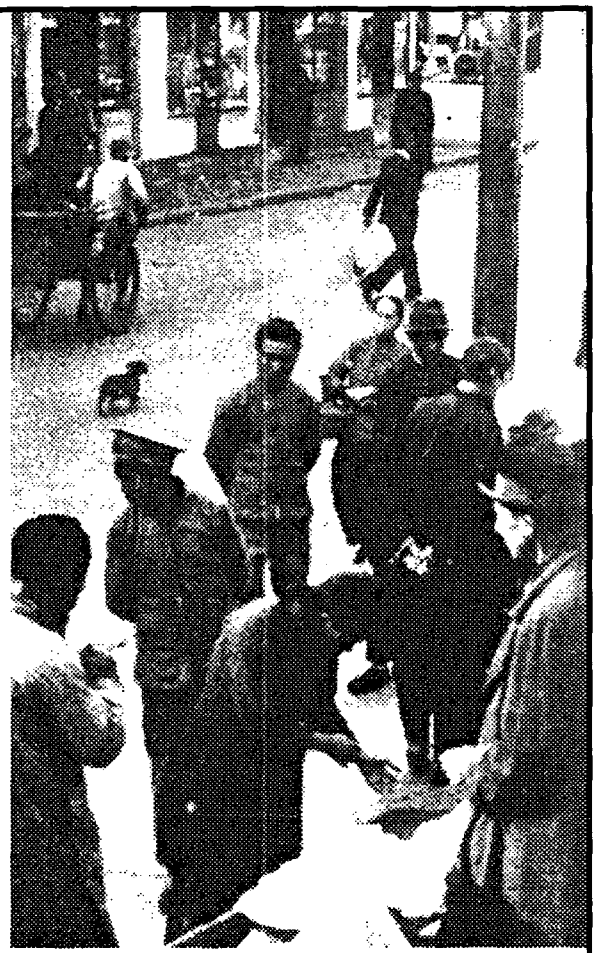
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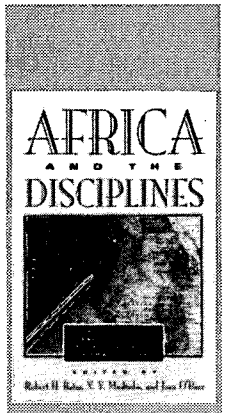
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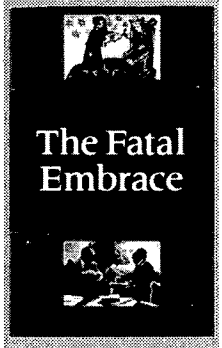
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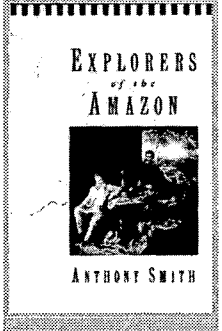
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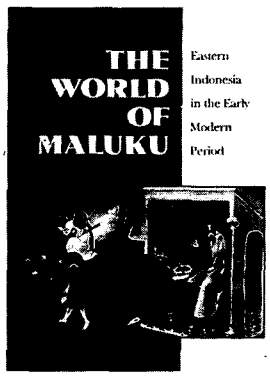
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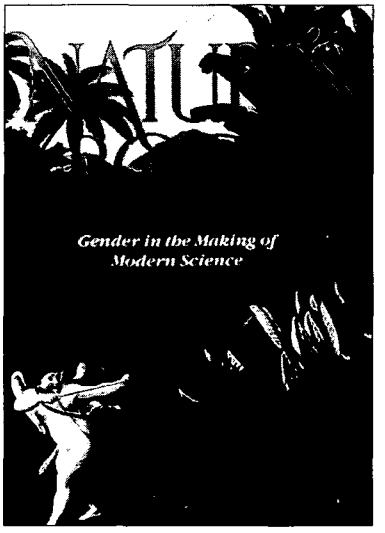
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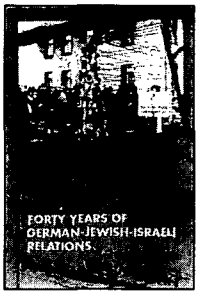
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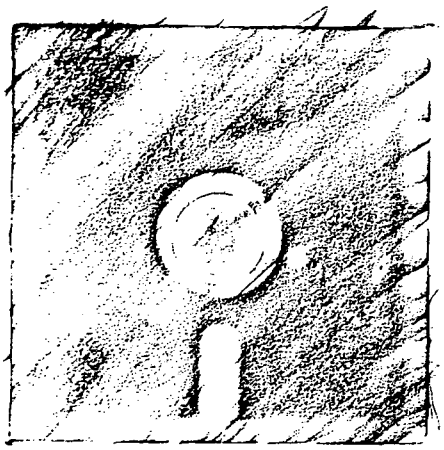
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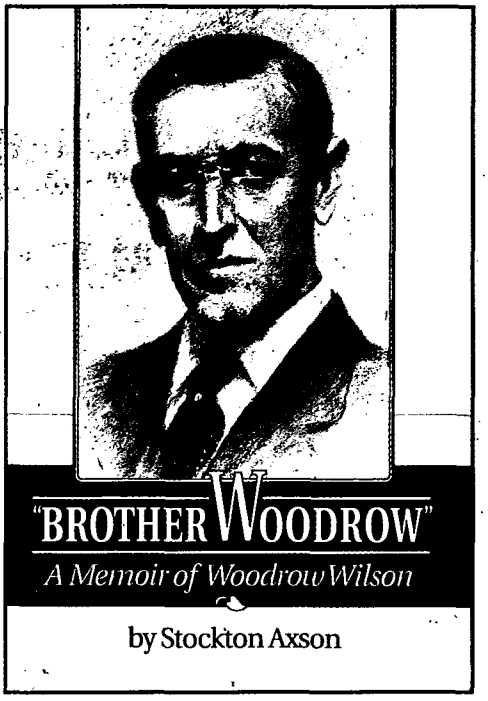
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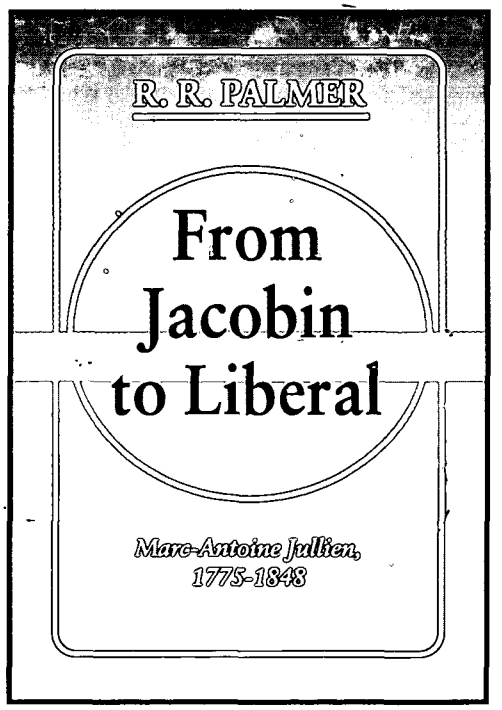
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